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A THEORY OF CIRCUIT INTEGRATION: A CRITICISM OF THE 'CENTRALLY AROUSED PROCESS'

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The supposition that there are centrally or cortically aroused processes has had a long and interesting history. It has received repeated but only apparent substantiation not only from experiments which have seemed to supply the necessary facts but from speculative deductions the plausibility of which has been accentuated not so much by facts as by the lack of contrary evidence.

With philosophical and epistemological distinctions between the subjective and the objective influencing psychological thinking from the outset and with a dualistic theory which associated mind with the former and matter with the latter, the way was paved at an early date for a persistent and vigorous faith in an 'inner world.' As soon as it was found that an injury or disease of the brain affected this 'inner world' there gradually crystallized the notion that subjective phenomena and the brain went hand in hand. So perfect has been the correlation, apparently, between mental and cerebral activities that the possible inadequacy of this fact as a scientific guide to a psychophysical theory has not been adequately inspected. Meanwhile we have been speaking of centrally aroused processes with almost as much assurance as a scientific verification justifies.[1]

It is not the intention of the writer to set forth too serious a substitute for the theory of centrally aroused processes. Sufficient evidence is not available. Rather it is the purpose of this paper to raise an issue and to offer a few suggestions toward

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[1]Cf. O. Kulpe, *Outlines of Psychology*, 1893, "the latter (centrally excited sensations) need no peripheral excitation. . . ." (p. 35). They may be correlated, however, with a co-excitation of peripheral organs (p. 85). See p. 184 for a defense of a theory of cortical sensations, and p. 272 for a central theory of feeling. Note also p. 441. W. B. Pillsbury, *Essentials of Psychology*, 1920, 141 f. Madison Bentley, *The Field of Psychology*, 1924, 213, 241, 252. H. C. Warren, *Human Psychology*, 1919, 148, 277.

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what might be called a 'total integration' theory. Throughout it should be borne in mind that no effort is made to belittle the importance of integrations to which the cortex unquestionably contributes important features. Specifically it is the prevailing implication that the brain functions under certain conditions independently of any peripheral stimulation that is here subjected to criticism.

We begin with one of the major considerations fundamental to the central origin of mental processes, the problem of the localization of function. It is hardly conceivable in the light of such information as we have that images of a specific quality and widely differing from one another in quality are dependent upon any part of the cortex that may happen to be functioning at the time the images appear. Quite as difficult is it to suppose that the brain as a whole subserves images of all modalities independently of the periphery. Either assumption begs too many questions. Perhaps it is reasonable to assume that the margins of the different sensory areas subserve imagery for the modality in question. But such a theory is open to serious question, as will soon be pointed out.

The confused state of our present knowledge with reference to localization of function is due, in general, to the prevailing tendency in human thinking to interpret two contiguous events exclusively as cause and effect. To destroy a certain region of the brain and forthwith to lack a certain mental process has been regarded as evidence that the former was the seat of the latter. Another source of error in thinking is the tendency to look for something real in entities, structures, stuffs, and the like, which has shown itself in an anatomical bias in our discussions of the reflex arc. With mental processes construed as structural entities and a tripartite conception of the reflex arc, it naturally followed that one must search for an anatomical place of abode somewhere along the arc. One structural notion begets another.

It is not surprising, then, that we should be called to task for our mistakes. Results of recent experimentation upon the effect of brain injuries on behavior in animals, especially Lashley's elaborate researches,[2] compel us materially to alter if not

[2]K. S. Lashley and S. I. Franz, The effects of cerebral destruction upon habit-formation and retention in the albino rat, *Psychobiol.*, 1, 1917, 71-139; K. S. Lashley, Studies of cerebral function in learning: I. *Psychobiol.*, 2, 1920, 55-135; Studies of cerebral function in learning: II. The effects of long continued practice upon cerebral localization, *J. Comp. Psychol.*, 1, 1921, 453-468; Studies of cerebral function in learning: III. The motor areas, *Brain*, 44, 1921, 1-31; Studies of cerebral function in learning: IV. Vicarious function after destruction of the visual areas, *Amer. J. Physiol.*, 59, 1922, 44-71; Studies of cerebral function in learning: V. The retention of motor habits after destruction of the so-called motor areas in primates, *Arch. Neur. Psychiat.*, 14, 1924, 1-28; Studies of cerebral function in learning: VI. The theory that synaptic resistance is reduced by the passage of the nerve impulse, *Psychol. Rev.*, 31, 1924, 369-375; Temporal variations in the function of the *Gyrus Precentralis* in primates, *Amer. J. Physiol.*, 65, 1923, 585-602; Studies of cerebral function in learning, *J. Comp. Neurol.*, 41, 1926, 1-58.

to repudiate entirely the notion of localization of function. After all, why should we have concluded that, because destruction of the occipital lobe was correlated with a loss of vision, this lobe contained the seat of vision? One might as well conclude that because he cannot see without a retina the retina is the seat of visual processes, or that the loss of cutaneous sensitivity in the hand when a nerve in the fore-arm is sutured proves that the seat of the cutaneous sensitivity in question is the suture. Destroy a single link in a chain and the chain ceases to function.

Lashley's results suggest that the main axones from the lower visual centers to the cuneus are merely the normal anatomical connections between the lower brain centers and the cortex and that the cuneus is a visual center only because the visual nerve currents happen to reach the cortex by that route. That is, the relation of this area to vision is merely that of an entrance-way to the cortex. The visual functions, presumably, do not terminate in this region. How other parts of the brain take over the lost receptivity function is not known but presumably as more devious routes take on the conduction of currents from the optic nerve.

Moreover, when we assume that a visual consciousness is subserved cortically by the visual area alone, we are at once confronted with the task of understanding why, as nerve currents reach that area, the visual process comes into existence and why, as the currents leave the area, visual processes cease to exist. Where are the peculiar structures at the boundaries of this area that account for these striking events? It would seem more plausible to assume that visual processes appear and disappear when a peripheral stimulus ceases, or when the visual response, subserved by action-patterns extending through the cortex, is somehow inhibited.

Further, why are there not distinguishing features at the boundaries of the different sensory areas that would account for the qualitative differences in the mental processes, and why is there not something observable between a so-called sensory area and its margin that would help us explain the differences between sensation and the image? Not only are such cues lacking but such a central theory presents its logical difficulties. These were pointed out long ago by Dewey.[3] By what logical prerogative do we justify the notion that covert behavior[4] is sensory and dependent upon the sensory segment of the reflex arc and that it is not motor and not dependent upon the motor segment of the arc? There seems to be no evidence from a

[3] John Dewey, The reflex arc concept in psychology, *Psychol. Rev.*, 1, 1893, 357 ff.; see also *J. Phil.*, 9, 1912, 664 f.

[4] R. H. Wheeler, Outline of a system of psychology, *Psychol. Rev.*, 30, 1923, 151-163.

functional point of view why we should adhere so strictly to such anatomical terminology. Exactly where, in the arc, do the motor functions begin? Movements are caused by stimulation of the so-called sensory areas! We have no evidence that the motor areas function independently of the sensory.

If the brain as a whole or at least the major portion of the cortex functions as an integrating region in the performance of a single act, no matter from what source the stimulation, it would seem as if the same conditions held for mental processes if the latter have anything to do with behavior. Then the requisites for brain functioning at any time when mental processes are involved, are not only that the brain as a whole be intact but that a certain balance of potentials obtain within the brain and between the brain and the periphery. Shut off all afferent impulses in any one pattern and presumably that pattern would be inert. In other words sensory and motor segments are serviceable only in explaining alterations and overlappings of patterns. The pattern in any one case is to be regarded as a whole from sense organ to muscle whether the central regions involved be large or small, simple or complex, and no matter how much the latter contributes in the control of the resulting response, and no matter how much it surpasses in area or bulk the nerve material at the periphery.

We are evidently dealing with a problem in dynamics. While the brain contains an unmeasured amount of stored energy, it is a question, from the standpoint of energetics, whether this energy is ever available except as disturbing or catalytic agencies reach it from without. While facilitating agencies may reach it from the blood stream, such influences seem hardly adequate to the setting of nerve impulses in action. We might expect easy or more difficult conduction according to the condition of the blood, but we should hardly expect a nerve current to arise except as the nerve itself is excited somewhere along its course. As far as we know the normal place for such a disturbance is the receptor. Granting the theoretical possibility that, since energy from the blood is worked over into nervous energy, the circulation of the blood in the central nervous system may excite mental processes, we are still confronted with the very difficult problem how it is that these circulatory changes are occurring continuously at the right time and in the right place! The alternative, of course, is to suppose that when there is no peripheral stimulation whatever either over extero- or proprioceptive channels, the brain is capable of discharging energy into the muscles, or of working over its energy in such fashion as to maintain covert behavior. But why impose upon the brain the embarrassing task of lifting itself by its bootstraps? If the brain does function as a unit

we should anticipate no expenditure of energy in the absence of a disturbance from without. Perhaps a little light is thrown upon the problem when we reflect that even in the absence of much extero-stimulation we have afferent avenues from the muscles, a situation which no doubt helps to supply the equilibrium or proper adjustment of potentials between the centers and the periphery. We know, also, that when we reduce stimulation of the sense organs and relax throughout the body, we approach a state of sleep.

Rather than to interpret the brain as a creator, the better working hypothesis would seem to be that of regarding it as a combined battery and amplifier which functions only when the circuit is kept open. While this analogy is intended only as a crude figure it may prove worth considering in the light of the discussion which follows.

We are informed that the sense organs are not only connected to the muscles of the body by way of the main sensory-motor routes but also by way of collaterals that form shorter circuits. Accordingly, upon the stimulation of a sense organ there is possibly a slight innervation of the musculature before afferent currents from the sense organs reach the higher centers.[5] Before the response takes place, whether it be covert or overt or both, there has presumably been a secondary sensory stimulation over proprioceptive channels. This may explain the presence of kinaesthesis in connection with all mental processes and harmonizes with the reflex-circuit concept as proposed by Dewey.

Results from experiments on the acquisition of skill point in the same direction.[6] In the formation of any overt habit not only are the pathways from the most used receptors to the most used effectors important but pretty much the entire nervous system. It is evident that in habit formation of practically any kind there is an adjustment of the whole nervous and muscular system. For example witness the disastrous effects of trying to shoot a rifle accurately while taking a different stance from that originally learned.

Couple this general consideration with the recent emphasis of the *Gestalt* school[7] that at any one time we are responding not to a single stimulus or to a limited group of stimuli even when we do nothing more than to look at a pair of gray squares, but that we are responding to a total situation. What we see is the direct product of a vast number of stimuli, and the re-

[5] C. J. Herrick, *Introduction to Neurology*, 1918, 342 ff., refers to Kappers, *Bericht d. d. III Kongress exper. Psychol.*, 1908; *Folia Neuro-Biol.*, 1, 1900, 507-532.

[6] John Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 293 f.

[7] Cf. K. Koffka, The psychology of perception, *Psychol. Bull.*, 19, 1922, 542; *Growth of the Mind*, 1925; Harry Helson, The psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223.

sponse, considered introspectively or otherwise, is the product of a very general adjustment on the part of the central nervous system, an adjustment which depends upon relationships obtaining between widely distributed parts.

If the total integration theory not only has logical but anatomical and experimental support it is worthy of more serious consideration. Indeed it has a profound significance for systematic psychology for it reveals a possible mechanism for the multiple stimulation from differing sense modalities that is apparently essential to the existence of a mental process, if the simplest mental process is a complex.[8] Moreover, it furnishes us with a reason for the fact that the interpretive element in the percept always involves kinaesthesis and is dependent upon kinaesthesis,[9] and shows how there is always peripheral stimulation in the development of meaning no matter how rapid or attenuated that development is. It is no longer necessary to speculate blindly on the alleged origin of integrations in the central nervous system. It ties up the facts of covert behavior with those of overt behavior and this seems reasonable if man is a biological phenomenon. Finally, it is a constructive substitute for the tripartite concept of the arc. In this view since covert behavior depends upon the functioning of the complete reflex circuit the former is not exclusively sensory or motor but both and not exclusively aroused by forces at the periphery or in the center but both. There is always an adjustment of the centers to afferent currents and of the effectors to efferent currents.

We now pass to the second main problem, namely that of imagery, the stronghold of the advocates of centrally aroused processes. That the image is a centrally aroused process seems to receive confirmation from a multitude of sources. An image, it is said, is not dependent upon the stimulation of any sense organ. We may have visual imagery of a red disk when there is no red disk before us. We may have olfactory imagery of a certain odor when the nose is not being stimulated. If the nose were being stimulated the resulting process is a sensation, not an image. Further, it is possible, apparently, to have kinaesthetic imagery of a leg-movement after the leg has been amputated. Drugs, such as hashish, cause certain images to appear and this imagery possesses certain peculiar properties of behavior. The cause must be central. Moreover, imagery lacks the localization, clearness, duration, richness of quality and the intensity of peripherally aroused processes.

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[8] R. H. Wheeler, Persistent problems in systematic psychology: II. The psychological datum, *Psychol. Rev.*, 32, 1925, 261ff.

[9] R. H. Wheeler, The development of meaning, this JOURNAL, 33, 1922, 223-233.

All this evidence, however, may be misleading. Qualities and other properties of sensation apparently depend upon specializations of structure in the end-organs and upon certain properties of the stimulus. Evidently these sources are responsible for corresponding peculiarities of nerve impulses passing into the central nervous system from the various peripheral organs. If all such nerve impulses are alike how is the central nervous system affected differently by the different stimuli? Of what use are the sense organs, anyway, if their specialization fits only the stimulus and has no relationship with the nerve centers? We may choose to regard the sense organ as the seat of the sensation in question but this view has difficulties too obvious to mention. Assuming that nerve impulses from the retina differ in wave form from nerve impulses from other sense organs and that these differences account for the distinguishing features of visual sensations the problem arises, where are we to find equivalent specializations of structure to account for like differences between visual and other modalities of imagery. This difficulty vanishes if such brain patterns as subserve imagery include afferent routes from the sense organs. It might be, of course, that once a central pattern is thus set up by the action of a sense organ there is a functional specialization that accounts for these differences in imagery, or there may be a structural differentiation which neurologists have not yet discovered. But this interpretation meets with the objection heretofore mentioned that, functionally, there is no set region where an exclusively visual function must end and an auditory function begin and no position along the reflex arc at which it is conceivable that a sensory function ends and a motor function begins.

Evidence that there is no peripheral stimulation at the sense organ in case of imagery is inconclusive for the further reason that under normal conditions most if not all of the sense organs are continuously functioning. This means that specialized end-organs are affecting the centers constantly and that this effect is going on while an individual is having imagery. It is by no means clear, for example, that the retina does not supply peripheral stimulation for visual imagery. Those born blind never have visual imagery and the usual explanation has been to the effect that the retina has not supplied the appropriate 'traces' in the visual cortex. Quite as possible is the reason that the blind have no functioning retinas by means of which the visual patterns are constantly being excited. We do not know that the adventitiously blind who have visual imagery do not also possess functioning retinas and that visual imagery declines as the retinas degenerate. On the other hand we do

know that many of those who have lost their sight are not strictly blind, though they cannot distinguish daylight and darkness, for they still see black or retinal grey and have entoptic colors. This correlation may mean nothing but it certainly sheds a new light upon the problem at hand, and the writer, at least, has not found an adventitiously blind individual still having visual imagery in whom it is evident, after investigation, that no functioning retina is left. If this situation should turn out to be true generally we can see how the brain, functioning as an amplifier and battery, makes over this crude 'material' from the retina with the aid of proprioceptive stimulation. Thus we have a possible physiological basis for imagery.

If resident sensations elicited by inherent retinal 'light' (and no one knows how long this light will last with a subject confined in absolute darkness) furnish the raw material for qualitative features of visual images, it may very well be that resident sensations in the other modalities function in like manner. We do not know that the adventitiously deaf do not lose their auditory imagery with total loss of hearing. We cannot trust the evidence from a single or even a small group of individuals, especially untrained observers, because there are many who are not deaf and insist upon having auditory imagery in which there is not an auditory factor which stands out as such.

Perhaps there is a cochlear 'tone.' Elaborate tests should be made on trained subjects whose organs of Corti are known to be destroyed; but unfortunately for science such observers are rare! Experiments in sound-proof rooms might furnish interesting results one way or the other, but unreliable results, since such rooms are seldom sound-proof. Moreover, the individual himself must breathe and his heart must keep going!

Genuine imagery of coldness and warmness should be very carefully studied under controlled conditions. It is much easier to image warm when the skin is warm, cold when the skin is cold. It may be doubted whether it is possible to have tactual imagery localized on an anaesthetic area. Do hysterical patients have tactual imagery localized on the 'insensitive' areas? If our thesis be true there would be no genuine tactual imagery from an anaesthetic area; and if our thesis holds for a few modalities it probably holds for them all.

We have additional clues. The types of imagery which prevail in mental life generally are from such modalities as are most obviously subject to continuous peripheral stimulation. Outstanding among these are the visual and kinaesthetic. This fact is quite as consistent with our proposal as with the view that the more frequent the peripheral stimulation the more ready to

function are traces in the cerebral centers. Moreover, if imagery depends upon minimal stimulations of the sense organs we have one tangible reason, at least, for the observed differences between sensations and images. These stimulations are approximately liminal and hence the resulting processes are fleeting, vaguely localized, of weak intensity and lacking in detail unless, at the same time, the brain's elaborative function, and proprioceptive stimulation are at a maximum.

Facts recently brought to light in studies of *Eidetiker* show that the eidetic image arises most readily when the subject fixates a background, preferably the one on which the picture was originally exposed or one like it.[10] Dreams usually lack color, a fact which fits in with our thesis perfectly. The readiness with which different kinds of images may be elicited through suggestion points to peripheral stimulation and to a close relationship in general between central and peripheral processes. This brings us to a consideration of the conditioned reflex as a mechanism which may furnish the required secondary stimulus that controls the time of appearance, the form and perhaps other properties of the image. Presumably at least one of these secondary modes of stimulation is always proprioceptive.

By means of the conditioned reflex, presumably, motor attitudes, gestures, inner speech, etc., become supplementary stimuli in the arousal of visual, auditory, and other non-kinaesthetic modes of excitation, functioning together with the weak excitations from the relevant sense organs. Likewise, by means of the conditioned reflex, a visual stimulus may become adequate as one mode of peripheral excitation for auditory imagery or an auditory stimulus may be one of the conditions for the arousal of a visual image, and so on. We may suppose that by such a substitution of stimuli a visual response, for example, drains from a pattern connected with the ear. Weak excitations from the ear supply the material that is to be amplified. Visual and kinaesthetic stimuli furnish the conditions under which this auditory imagery takes on localization and form, and under which it possesses durative and clearness aspects.

Our theory proposes, therefore, that there are two kinds of peripheral stimulation in the case of every image, the first from the sense organ of the same modality as the image, and the second from one or more other modalities, one of which is always kinaesthetic. A given muscular adjustment may, in this view, be the adequate stimulus for a variety of images, depending upon the temporary alignment of patterns through the central

[10] E. R. Jaensch, cf. *Zsch. f. Psychol.*, volumes 84-92, 1920-1923.

nervous system. There need not be a muscular contraction of a specified amount, vigor and intensity for every specified property of the image.

So far we have stated our theory and have considered some of the objective evidence. We now turn to the evidence from introspection. While such evidence is still far from complete we submit that there is nothing in it that justifies a theory of centrally aroused processes. The sort of introspective evidence to which we refer, however, is furnished not by the verbal report method but by 'complete' introspection. It has already been pointed out that only the complete method furnishes data adequate for purposes of systematization.[11] We pointed out that the complexity of the simplest existing mental process has been overlooked. It is in this inadequately analyzed pattern that we may look for the introspective cues which point to a total-integration theory. One of these cues is the much neglected and at the same time much abused component, kinaesthesis. Another is the synaesthetic character of the simplest mental process. The first points to a peripheral factor, proprioceptive stimulation; the second, to innumerable possibilities in peripheral stimulation by way of the conditioned reflex. It is not improbable that the stimulus situation which arouses an auditory image, for example, is in part, a visual perception of an instrument, a singer, a word, a musical note or what not; or it might be a verbal image with its incipient movement and shifts of tension in the larynx. The combinations and possibilities are legion.

Evidence that all of the so-called centrally aroused processes contain kinaesthesis is becoming so significant that one would be justified in the case of negative results in looking for a substitute that owed its origin to the conditioned reflex. Visual synaesthesis is a case in point. The evidence, however, is scattered;[12] much of it is a by-product of other purposes and not properly checked. Few indeed have been the researches which can be considered reliable on this point. The chief reason for

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[11] R. H. Wheeler, Some problems of meaning, this JOURNAL, 34, 1923, 185-202, especially 195 ff.

[12] R. H. Wheeler, Visual phenomena in the dreams of a blind subject, *Psychol. Rev.*, 27, 1920, 313-322; The synaesthesia of a blind subject, *Univ. of Ore. Publ.*, no. 5, 1920, 1-61; R. H. Wheeler and T. D. Cutsforth, The synesthesia of a blind subject with comparative data from an asynaesthetic blind subject, *Univ. of Ore. Publ.*, no. 10, 1922, 1-104; The rôle of synaesthesia in learning, *J. Exper. Psychol.*, 4, 1921, 448-468; Synaesthesia and meaning, this JOURNAL, 33, 1922, 361-384; Synaesthesia, a form of perception, *Psychol. Rev.*, 29, 1922, 212-220; Synaesthesia in the development of the concept, *J. Exper. Psychol.*, 8, 1925, 149-159; T. D. Cutsforth, The rôle of emotion in a synaesthetic subject, this JOURNAL, 36, 1925, 527-543. S. C. Fisher, The process of generalising abstracton and its product, the general concept, *Psychol. Monog.*, 21, 1916, no. 30, 1-213; S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, no. 117, 1-161; E. L. Woods, An experimental analysis of the process of recognising, this JOURNAL, 26, 1915, 313-387; R. H. Wheeler, An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, no. 2, 1920, 1-59; H. R. Crosland, A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 29, 1921, no. 130, 1-159.

this is the fact that introspective observers have been slow to report kinaesthetic processes. It is not safe to conclude that, because some observers give negative results, there must be individual differences. The presence of kinaesthetic factors reported in a certain number of visual images, for example, is legitimate evidence pointing to the probable existence of such factors in all visual imagery in the same subject. There have been many observers who have failed to recognize delicate shifts and tensions in minor muscles, or who fail to recognize very weak and diffuse shifts more widely distributed over the body. Our muscles are never entirely relaxed and never absolutely quiet in waking life. When unrecognized these kinaestheses unquestionably play a part in the undifferentiated and unanalyzed background of the observer's experience. Thus, in case of visual imagery, incipient shifts of accommodation, changes in tension toward convergence and divergence—strains insufficient to produce observable eye-movement in any direction—have been overlooked either through lack of introspective training or for the want of the proper mental set.

In this connection it is easy to be led astray by objective methods that produce negative results. It is hardly safe to conclude that because we have failed to record eye-movements objectively the muscles of the eyes are not functioning in giving to visual processes the kinaesthesis to which a complete introspection testifies. It is quite as easy to make mistakes with an objective as with an introspective method when all of the conditions of experimentation are not controlled, and it is impossible to control them all in a psychological experiment. The ideal procedure of course, is to attack such a problem with both methods. That this is of paramount importance for accurate results is indicated by some recent work of Bott on the mechanics of continuous flexion and extension at the wrist.[13] The curve obtained by having a recording device attached to the fingers shows a gradual rather than a sudden shift from flexor to extensor movement. If flexion ceased before extension commenced movement of the hand would cease momentarily but this is not the case. Evidently, as Bott suggests, one phase of the movement begins before the previous phase ends; that is, extensor muscles begin to contract before the opposed set reaches its maximum of relaxation, and vice versa. This means that there is a tendency toward movement in one direction while observable movement continues in another. The writer has frequently observed qualitative differences in kinaesthesis which are presumably to be interpreted in just this way.

[13]E. A. Bott, Some characteristics of voluntary wrist action, *Brit. J. Psychol.*, 14, 1923, 1 ff.

There is little doubt that objective records alone will not tell us everything that is going on in musculature during overt action. It is conceivable that in many instances and under a variety of conditions there will be tendencies toward movement strong enough to be perceived, as a result of proprioceptive stimulation, but not strong enough to change the direction of movement. Moreover, it is possible that complex movements, resulting from a combination of stresses and strains, yield objective records that could not possibly agree, on superficial analysis, with careful introspective reports. This pertains particularly to eye-tensions, where there are several sets of muscles functioning in a multitude of ways. While studies like Dodge's,[14] for example, are valuable for many purposes, they cannot be accepted as reliable evidence that kinaesthetic sensations from eye-muscles are illusory because no objective record is obtained.

At first blush the problem with respect to auditory imagery would seem more difficult but here again the complete introspective method may yield significant results. There are incipient strains characteristic of auditory attention. These may be in the throat or in the superficial muscles of the neck or both, and even in the chest. It is to be questioned seriously if the so-called 'surrogates' which 'accompany' auditory imagery are surrogates at all. Rather they are part and parcel of the experience called an auditory image and if the 'stimulus-error' be not committed too flagrantly they may turn out to be the most important part of the image. The majority of observers with whom the writer has been associated are either kinaesthetically or visually synaesthetic to a degree. The intensity, pitch, and volume of tone, even quality, is invariably perceived partly at least in terms of visual or kinaesthetic processes, and attempts to ignore the latter merely drive them into a synthesis in which the complex is perceived in unitary fashion as auditory. The peripheral cue, it would seem, is not hard to find in the kinaesthetic element of the complex or in the various eye-adjustments made in connection with the visual component and in a tendency to schematize and localize the sound. It is surprising how a naïve or stubborn observer will deny these associates and at the same time falter over his descriptions of the auditory experience. Until he discovers these so-called associates he simply cannot give an account of the imagery. Working under the theory of 'givens' or 'existentiality' a trained subject is likely to dodge the issue by saying: "I was immediately

[14]Raymond Dodge, Five types of eye-movement in the horizontal meridian plane of the field of regard, *Amer. J. Physiol.*, 8, 1903, 307-329; An experimental study of the ocular reactions of the insane from photographic records (in collaboration with Allen Ross Diefendorf), *Brain*, 31, 1909, 451-489; A mirror-recorder for photographing the compensatory movements of closed eyes, *J. Exper. Psychol.*, 4, 1921, 165-174.

aware of the auditory quality as such." Then, perchance, he makes a complex out of it by enumerating its characteristics!

In the case of olfactory and gustatory imagery there is again a wealth of visual and kinaesthetic associates which ordinarily passes unnoticed. The musculature of the nose may tighten; there may be slight shifts in the tonus or lie of the chest musculature, tongue movements, contractions at the back of the mouth and in the throat, eye-movements connected with visual imagery or with localizing the image in question, or what not. Likewise in dreams. We cannot be certain that because we remember only the visual, auditory and verbal contents of a dream, other components are not lost in the relatively undifferentiated experience.

The problem of identifying an image of a specific modality is of course a very difficult one. It is doubtful if much that is ordinarily regarded as imagery of a certain kind is really that kind of an image at all. Indeed there is very likely no pure imagery of any kind. The best one can do is to assume as analytical an attitude as possible and accumulate masses of data from the greatest possible number of subjects. Confusion may arise with embarrassing readiness in describing images that are elicited with great difficulty. A flashy and fleeting experience may possess the meaning cold, but when analyzed carefully there may be hardly any quality of coldness present, and the flashiness turns out to be a name for the visual component! The writer has known synaesthetic individuals who have lived for years unaware of the fact that their cutaneous images were more visual than cutaneous. The experiences meant cold well enough and in fact to them were cold.

Doubtless the so-called asynaesthetic individual will find himself to a degree in the same predicament with the exception that he can pick out the tactual feature more readily because the interpretive or recognitive phase of his awareness has become integrated with the tactual part of the complex as well as with the visual and other 'associates.' But when it comes to a definite description of this tactual component, as such, he is quite as much at a loss as the obviously synaesthetic individual. He inevitably uses the associates and certain of these are bound to be easily recognizable as sensory.

Indeed, our treatment of imagery confessedly makes of it a weak or partially aroused sensation.[15] But now we must not consider that, because of this, images depend upon peripheral stimulation exclusively. Even in the case of sensation it is plain that central integrations are essential. The fact that sensation is conditioned by peripheral stimulation does not

[15] R. H. Wheeler, Outline of a system of psychology, *Psychol. Rev.*, 30, 1923, 157 ff.

make of it a peripherally aroused process and nothing more. Sensation is quite as much a central as a peripheral process. While this may seem to be a trivial point it is necessary, for the sake of clearness, that we carry the logic of our position to its conclusion. Proportionately there should be no more peripheral stimulation in the case of sensation than in the case of imagery. Conversely, we may doubt whether in the one case there is proportionately more central activity than in the other.

We should not leave out of account the various abnormal mental phenomena which the critics of a total-integration theory are likely to mention as damaging evidence. How, for example, does post-hypnotic suggestion depend even in part upon peripheral stimulation? Certainly the person who made the original suggestion need not be present at the time the instructions are carried out. Let us suppose that a patient is told, under hypnosis, that he will arise from the dinner table while at dinner, put on his hat and coat, and leave the house. A possible source of peripheral stimulation has already been mentioned—the dinner table. Presumably it would not have functioned as the peripheral stimulus had there not been the proper nervous set. A total-integration theory takes this into account. It supposes, however, that nervous sets of any kind never result in action—indeed, do not exist—in the absence of an adequate objective situation. Any detail of the patient's environment at the time he fulfills the suggestion, duplicated in the situation in which the original suggestion was given, may, through a conditioning process, function as an adequate peripheral stimulus. We have no way of measuring the far-reaching influence of conditioned reflex phenomena, or the extent to which two apparently different situations contain stimuli in common. If it is true that every time we respond we are reacting to a total situation, including proprioceptive stimulation, it is bewildering to reflect upon the number of possibilities whereby an apparently irrelevant item may be playing an important rôle in behavior. Let a single contributing stimulus be represented twice, in two successive responses, and we have the possibility of the peripheral cue for which we may be hunting. The horse that shied apparently for no particular reason, while being driven along a country road, was responding to a substitute stimulus present in the original setting when a newspaper blew across the road.

We have touched upon only a few of the problems confronting the advocate of centrally aroused processes. Those who, in the earlier period of experimental psychology, upheld the innervation sense, relied upon superficially analyzed and mis-

leading evidence. The theory that the *Aufgabe* and its determining tendency represent central innervation mechanisms rests upon a foundation equally questionable. The writer has already suggested,[16] upon the basis of introspective data, that a mental set is essentially a peripheral innervating mechanism the evidence for which lies in the presence of kinaesthesis. The *modus operandi* of the determining tendency is a definitization and elaboration of an initial and relatively undifferentiated motor adjustment, the stages through which the definitization and elaboration pass being dependent upon supplementary stimulation.

Let a subject attempt to hold a visual image before him and he finds that secondary expedients are necessary, such as moving the image about, changing its shape, using verbal suggestions and the like. In the voluntary control of movement one's attention is centered upon the object and upon his muscles. We are forced on every hand, therefore, to conclude that voluntary phenomena are quite as much peripheral as central.

The problem of attention, like the problem of voluntary control, should not detain us long. So-called motor accompaniments of attention have long been recognized. Why they were defined as accompaniments rather than as genuine phenomena of attention is traceable to the dogma of parallelism and the doubtful statement of fact that attention precedes the motor accompaniments. The latter declaration assumes that a process appears and is attended-to afterwards, a statement that is neither consistent with the facts nor logically pertinent. It is both in the adjustment to attend and in the act of attending that we find kinaesthesis. Every shift of attention involves motor adjustments. The evidence is well worth considering, therefore, that one of the essential conditions of attention is peripheral stimulation. Then, of course, if our previous suggestions with respect to perception and image are correct it follows automatically that attention is a peripherally as well as a centrally controlled adjustment.

Beginning with Plato, if not earlier, theories of memory have repeatedly been couched in terms of central traces or residues. From the time of Aristotle it has been customary to speak of association of ideas as if one idea literally evoked another. Either this implication has been forestalled by a guarded qualification to the effect that the real dynamics of the process is to be found in the brain, not in the idea, or the issue has been clouded by a reference to the subconscious. As a result of

[16] R. H. Wheeler, An experimental investigation of the process of choosing, *Univ. of Ore. Publ.*, no. 2, 1920, 1-59.

this long history a set of terms such as recall, revival, reinstatement, and the like, have attained the status of common usage, yet there is nothing in these terms that indicates the nature of the actual processes involved. Such a begging of the question is not necessary under the total-integration view. Images are incomplete sensations; ideas are incomplete perceptions. Association of ideas becomes a highly modifiable and easily inhibited seriation of sensory-motor reactions, one reaction furnishing a peripheral stimulus, through proprioceptive stimulation, for the next. The direction in which a course of ideas may lead obviously depends upon the interrelations of action patterns but we are at a loss to know how these patterns function in the absence of peripheral influences.

The problem of affection has played a prominent rôle in the development of cortical theories of mental processes. We can touch upon the matter only briefly. Suffice it to say that everything said up to date about affection can be said of kinaesthesis. It remains for more careful introspection to settle the question. It is claimed, for example, that affection lacks clearness.[17] So does a diffuse kinaesthesis or organic process or for that matter any mental process a complete cognition of which is prevented by an absence of the proper cues or setting. We are here involved in the distinction between attributive and cognitive clearness which we have already commented upon.[18] Affection may lack localization. So may kinaesthesis. We have qualitative opposites in affection, likewise in kinaesthesis—strain and relaxation. It has been suggested that affection may depend upon certain undifferentiated nerve endings in the body. These may be classified as kinaesthetic. Affection is characterized as subjective. What could be more subjective than a vague, diffuse and unlocalized kinaesthesis? We do not wish to imply that every experience of pleasantness and unpleasantness can be reduced exclusively to kinaesthesis. No complex experience can. Certain observers may find that pressure or some organic complex may constitute affection under certain conditions. It all depends upon the observer's mode of interpretation and upon the history of pleasantness and unpleasantness in his particular case. Moreover, there are many reports of affection that turn out to be nothing other than judgments of like and dislike, attitudes of acceptance and rejection, for the most part verbal and otherwise motor in character. These responses possess no content which, in any other setting, would mean pleasantness or unpleasantness. We are inclined to hazard the suggestion,

[17] E. B. Titchener, *Text-book of Psychology*, 1909, 213.

[18] R. H. Wheeler, Some problems of meaning, this JOURNAL, 34, 1923, 196; Persistent problems in Systematic Psychology: II. The psychological datum, *Psychol. Rev.*, 32, 1925, 451 ff.

therefore, that there is nothing known about affection that is incompatible with the thesis of this paper. The case of emotions is too obvious to require comment.

To recapitulate, we have raised the systematic issue in this paper whether centrally aroused processes exist and our negative conclusion rests upon indications from experimental work on the brains of animals, the circuit concept of the reflex arc, the continuous functioning of the sense organs, the prevalence of kinaesthesis in mental life, the principle of the conditioned reflex and the method of complete introspection, and the evidence that when we respond overtly or perceptually we are reacting to a stimulus-situation. We have brought to bear upon the problem the difficulty of describing functions in terms of fixed and stationary structures in such a way as to imply that the function is derived from broken continua, and the difficulty of implying that energy is created in or discharged from the brain without outside help.

The view here suggested assumes that integrations do not arise in the cortical centers but are functions of complete reflex circuits and of continuous peripheral stimulation from more than one sense modality, one of which, however, is always kinaesthetic. This view does not minimize the importance of brain centers but implies that they do not function independently of receptors and effectors. The brain is regarded as an amplifier and battery whose activities depend upon an open circuit. The sense organs are regarded as necessary for the supplying of those differentiated energy-patterns which result in the appearance of images of different qualities. Substitution of stimuli and conditioned reflex phenomena, operating especially through the medium of proprioceptive stimulation are suggested as the conditions for the time of appearance, the duration, form and 'behavior' of images. Because all such integrations as presumably subserve mental life in any of its forms are neuro-muscular integrations, based upon complete reflex circuits, we have called our hypothesis a total-integration theory of mental processes. It is strictly opposed to the prevailing conception that certain mental processes are cortically or centrally aroused.

THE INTELLIGENCE FACTOR IN THE SOLUTION OF SPACE PROBLEMS WITH THE TWO STORY MAZE

By CATHARINE M. COX, Stanford University

I. PROBLEMS AND APPROACH

Is the adequacy of adjustment or procedure in situations involving new motor space experiences conditioned to a greater extent by imagery type or by intelligence? What is the rôle of each in solving problems in motor space? The two-story maze offers an opportunity for the observation of individual differences in methods of attack on motor space problems.[1] Variations in time and error scores and in cue pattern on a series of five motor space problems have been observed, and in connection with the gathering of these data the parts played by visual and kinaesthetic factors have been examined to some extent. Learning was not primarily involved in the problems presented, but by reason of the serial method of presentation the learning factor inevitably entered in.

The extent to which the intelligence of the *S*s conditioned the building up of successful procedures, and the relation of intelligence to (1) speed in solving double-maze problems, (2) number of errors, (3) type of cues and imagery, and (4) number of components in a cue scheme, have been considered and some results are available on these points in answer to our double question.

Apparatus. The Miles two-story duplicate maze has been found peculiarly adapted to the investigation of motor space procedure. It presents a situation in which visual and kinaesthetic experience are combined, but not in the usual manner, for here visual guidance is indirect. The proportion of the relative participation of visual and kinaesthetic cues may here be altered from problem to problem by changing the position of one of the mazes. The same maze pattern is retained throughout the experiment. The apparatus consists of a combination of two identical stylus mazes set one directly above the other The upper one is seen and traced with one hand, while the lower one is traced by the other hand out of view. In Plate I an *S* may be observed in position at the maze. The picture was taken from the position of *E*.

The motor space problem series. Five positions of the lower maze, the upper remaining fixed, formed the series of problems presented for solution to 67 university students, members of a class in elementary psychology.

*Accepted for publication December 20, 1927.

[1] Walter Miles, The two-story duplicate maze, *J. Exper. Psychol.*, 10, 1927, 365-377.

PLATE I. AN *S* SOLVING PROBLEM 2 ON THE MILES TWO-STORY DUPLICATE MAZE

Problem 1. 0°. The lower maze in register, *i.e.* in the same position as the upper (visible) maze.

Problem 2. 180°. The lower maze revolved half way, so that the side that was nearest the *S* is now farthest from him.

Problem 3. 90°. The lower maze is turned one quarter so that its paths are at right angles to their original position and to those of the visible maze.

Problem 4. 180°. (reversed) The maze is turned over on its face so that the side nearest the *S* is now farthest from him, and the maze offers a mirror presentation of the pattern.

Problem 5. 90°. (reversed) From the position as in Problem 3, the maze is turned over on its face. Also a mirror position.

This series was not intended as a learning experiment, but the learning factor entered in largely in proportion to the length of time required by the *Ss* to solve the problems. Those whose method of procedure was sufficiently successful (and adequately combined with motor quickness) completed the whole series so promptly that the learning factor was slight. Those who proceeded slowly on the first problems learned, or partially learned, the maze pattern, and so their performance on the later problems was proportionately faster.

In the course of the investigation striking differences appeared: (1) in the time required for the solution of the various problems in the series; (2) in the type of motor-sensory guidance which the *S* believed he was most dependent upon, and in the type of his imagery; and (3) in the extent to which he employed logical or rational processes or guides, and the nature of these. In another place these three kinds of differences are discussed for the group as a whole, and the time scores for two sub-groups, the two sexes, are compared.[2]

Variations in all three of the aspects of behavior observed in this motor space problem situation, *i.e.* differences in the time scores, in motor-sensory cues and imagery, and in rational analysis, brought up the question as to the extent to which the presence and influence of the variable factor of intelligence was operative.

The Intelligence Element. The purpose of the present study therefore has been to discover the relation of intelligence to procedure in a series of five motor space problems.

The intelligence of our *Ss* was measured by score on the Thorndike college entrance test. A further criterion of ability was also available: the term grade of each of the *Ss* in elementary psychology. On the basis of the Thorndike scores the *Ss* were classified into three groups. The behavior of these groups described in terms of procedure in the maze problem situation and certain characteristics of the three intelligence levels are presented. Time and error scores are compared with intelligence rating, and the types of imagery and other cues employed by the different intelligence groups are considered.

II. Other Studies

A number of investigators have considered the effect of intelligence (1) in motor space problem learning, and (2) in problem solving of various types, but not, so far as the writer is aware, in motor space problems like the present ones that are not primarily problems in learning. Gould and Perrin concluded from their study of the maze learning of human adults and

[2] C. M. Cox, Comparative behavior in solving a series of maze problems of varying difficulty, *J. Exper. Psychol.*, 11, 1928, 202-218.

children that behavior in the initial attack on the maze problem gave "a graphic representation" of intelligence measured by chronological age.[3] The adult of 21 they found superior to the child of 11 in learning the pencil maze, when scores on errors, time, and distance were compared. But they concluded further, that "all distinctions found to hold between the child and the adult are extremely relative. Some of the children seemed to compare in every respect with the normal average adult" and the reverse was also found to be true. Hicks and Carr compared three intelligence levels, those of rats, children, and adults.[4] They found that certain features of the numerical expression of the learning processes in the maze could be isolated to serve as indices of intelligent behavior. An inverse relation appeared when degree of intelligence as shown by the three groups was compared with surplus time and error elimination.

Gopalaswami found that the success of 10 *Ss* in learning the solution of new motor space problems correlated with teachers' estimates of intelligence, 0.81 in one case, 0.05 in another.[5] (The author notes that the groups are too small for correlations to be particularly significant). The problem was mirror tracing of the star pattern: in the first instance the usual simple method was used, in the second an apparatus was employed that eliminated the *S*'s knowledge of his errors. Learning in this case showed practically no correlation with estimated intelligence.

Hunter compared maze performance with scores on the Otis intelligence test for 59 *Ss*.[6] Various measures of time (total time, time of one trial minus another, time of each of ten tenths of all of the trials), measures of total trials, and total errors were correlated with Otis scores. All of the correlations were negative, thus showing correspondence between speed in movement, speed in learning, and brightness. The correlations range from -0.02 ± 0.10 (intelligence and time on the 10th tenth) to -0.45 ± 0.08 (intelligence and total time). Fairly comparable with the present study (because they indicate the relation of intelligence to initial problem attack) are the following correlations on early performance in maze learning: intelligence and time of the first trial, $r = -0.22 \pm 0.06$; intelligence and time of the best of the first three trials, $r = -0.33 \pm 0.087$. Total time appeared to be the best single indicator of ability ($r = -0.45 \pm 0.08$), considerably exceeding total trials ($r = -0.17 \pm 0.094$) and somewhat exceeding total errors ($r = -0.34 \pm 0.087$) in correlation with Otis score.

The four investigations referred to are illustrative of those primarily concerned with the relation between maze *learning* or puzzle solving and intelligence, and yet they have thrown, as well, some clear light on the question of the relation between intelligence and *attack* on a maze problem, *i.e.* procedure in the early trials before learning is largely involved. Particularly significant for us is the finding that behavior in the early trials did give a 'graphic representation' of intelligence and that the time on early trials as well as total time, showed a definite negative correlation with the intelligence score.

A second group of studies that concern us are those which consider the relation of intelligence to problems of various sorts that are not primarily learning problems. From investigators of performance tests of intelligence we have Pearson's correlations between group intelligence scores and time scores on puzzle problems. Somewhat comparable to our problem series in that they present novel puzzle situations are the Ruger

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[3] M. C. Gould and F. A. C. Perrin, A comparison of the factors involved in the maze learning of human adults and children, *J. Exper. Psychol.*, 1, 1916, 122-154.

[4] V. C. Hicks and H. A. Carr, Human reactions in a maze, *J. Animal Behav.*, 2, 1912, 98-125.

[5] M. Gopalaswami, 'Intelligence' in motor learning, *Brit. J. Psychol.*, 14, 1924, 274-290.

[6] W. S. Hunter, Correlation studies with the maze in rats and humans, *Comp. Psychol. Monog.*, 1, 1922, 37-56.

wire puzzles, successful solutions of which were found by Camp Dix examiners to correlate 0.37 with Army Group examination Alpha.[7] Porteus' pencil mazes (Camp Dix results) correlated 0.46 with Alpha.[8] Correlations between puzzle problems and individual intelligence test scores (mental ages), or with performance test scores, are often considerably higher. The Porteus maze is an example of a single performance test that correlates as high as 0.46 with a group test of intelligence (the correlation between Porteus and Stanford Binet was 0.66, Army results), and has a distinct contribution to make to a more extended performance test scale (as in Beta).[9]

The Miles double-maze test has not been standardized as a performance test of intelligence, but the results obtained in the present study suggest that one of the 5 problem positions with a preliminary practice position might make an interesting and serviceable addition to the available group of performance tests.

III. Intelligence Scores of *Ss*

During the progress of the experiment the Thorndike scores were not known to *E*. They were not considered until after the other data had been evaluated. The Thorndike scores were found to cover a range of 70 points from the lowest score, 37.29 to the highest 109.90. The mean score was 72, median 72, standard deviation 16.50.

Table I

Correlation of Intelligence Scores with Time Scores on Motor Space Problems

Problem	Correlation Coefficient
1, 0°, register	−0.26 ±0.06
2, 180°, one-half revolution	−0.59 ±0.05
3, 90°, one-quarter revolution	−0.40 ±0.07
4, 180°, reversed	−0.11 ±0.09
5, 90°, reversed	−0.09 ±0.09

IV. Intelligence, Time and Error Scores

When individual time scores on the separate problems were correlated with Thorndike scores, the results in Table I were obtained.

The first three positions show a fair negative correlation between slowness and brightness. It may be seen that the time score on Problem 2 appears to compare favorably, as an intelligence measure, with some performance test problems in general use. The five correlations are all obviously influenced by factors of difficulty and position of the problem in the series.

Each of the five motor space problems presents a different situation as far as intelligent behavior is concerned. We have attempted to analyze each from this angle, and to point out

[7] Psychological examining in the United States Army, *Mem. Nat. Acad. Sci.*, 15, 1921, 363.
[8] *Loc. cit.*
[9] *Op. cit*, 404.

some of the elements common to two or more of the problems that do not appear in the others. Table II, a table of correlations, shows these comparisons.

Problem 1. The two mazes are in exactly the same rotation position. One might expect this problem to be too simple to bring out differences in intelligence in college students. Probably acuteness of visual imagery, general liking or disliking of puzzles, simple manual dexterity, the kind of simple coördination in similar movements common in many every-day activities are measured here rather more than intelligence. Still there is a low but significant agreement between speed and brightness. (Average time for 67 *Ss* equals 96 sec.)

TABLE II

INTERCORRELATIONS OF TIME SCORES ON FIVE SPACE PROBLEMS

Problem	Problem				
	1	2	3	4	5
	(67 cases)	(67 cases)	(64 cases)	(59 cases)	(55 cases)
1	—				
2	0.47±0.05	—			
3	0.17±0.07	0.39±0.07	—		
4	0.46±0.07	0.43±0.07	0.34±0.07	—	
5	0.02±0.09	0.10±0.09	0.30±0.08	0.11±0.09	—

Term grade in Psychology and Problem 2, r = −0.25±0.06.
Term grade in Psychology and Thorndike score, r = 0.50 ± 0.06.

Problem 2. Here the lower maze is turned half-way round. This position might be expected to test intelligence more definitely than that of Problem 1. A reasoned method tended to produce a successful procedure in this case. The lack of a rational plan left the *Ss* no other method than trial and error. Apparently the time at which a logical or rational method began to control the procedure depended upon the grade of intelligence of the *S*. This problem seems to measure the same qualities enumerated for Problem 1; in addition it requires quick analysis of a more unusual situation, but one that is not infrequently met in every-day life. The similarity of 1 and 2 appears in the correlation of their time scores (+0.47 ±0.05).

Problem 3. A quarter turn. At this point the influence of learning probably begins to make itself felt, especially in the case of some of the *Ss* who were slowest on Problem 2. For the individual who reasons, a quarter-turn is more difficult than a half-turn (Problem 2), or than a half-turn reversed (Problem 4). We are not accustomed to thinking in terms of quarter-turn adjustments. Visual imagery when available, probably aids reasoning at this point. The *S* who gets along as best he may by 'trying' or 'feeling it out' may 'know' the maze better from his long experience with Problem 2. He may therefore perform this really more difficult task in even less time than he required for 2. The relation between 3 and 1 or 2 has then two contradictory elements: (a) for the reasoner, 3 is quite a different problem from, and a much harder one than, 2 or 1; (b) for the kinaesthetic learner the problem is similar to 2 and 1 and easier than these because of learning through practice on them. Inasmuch as most individuals are partly reasoners and partly kinaesthetic learners, these two contrary principles may be assumed to be at work in greater or less degree in all cases. As a logical problem, 3 is less like 1 than 2. The correlation of 1 and 3 shows this dissimilarity increased perhaps by the learning element in 3 (+0.17±0.07), while 2 and 3 give clearer evidence of their relationship, (+0.39±0.07).

Problem 4. After a half-turn the maze is turned on its face. This position is more like 1 and 2 than it is like 3 or 5. It demands the same analytical response as that which proved effective in 2, but here even more than in 3 learning enters into the results, simply because of the position of this problem so late in the series. We may, therefore, expect some of the *Ss* who spent more time on 2 and 3 to solve 4 more quickly in proportion than other individuals do who are proceeding by a more rational method. Because of its essential characteristics as a motor space 'maze problem' 4 correlates positively with 1 ($r = 0.46 \pm 0.07$), 2 ($r = 0.43 \pm 0.07$), and 3 ($r = 0.34 \pm 0.07$); because of its greater similarity to 1 and 2 the time scores in these cases agree more closely.

Problem 5 After a quarter-turn, the maze is turned on its face. This problem is the hardest of the five for rational solution. It is like 3 in the awkwardness and unfamiliarity of the quarter-turn situation. But in its position in the series the element of motor space learning is pronounced, especially for the slowest individuals, and this obscures the effect of the rational difficulty of the problem. A number of the slowest workers have dropped out leaving contrasts in time not so great as they would otherwise probably be.

Problem 2, preceded by Problem 1, is the most satisfactory as a measure of general ability as scored on the Thorndike test ($r = -0.59 \pm 0.05$). Thorndike score and term grade in elementary psychology correlate $+0.50 \pm 0.06$. Time scores on Problem 2 appear to measure in large part something different from the ability necessary to succeed in elementary psychology as measured by term grade ($r = -0.25 \pm 0.06$).

There are two technical factors which, beside unrelatedness of function, make for low correlations between intelligence and speed in this experiment series. First, the fastest workers can not go much faster by reason of learning. A certain amount of time is required to go through the motions; even knowing the maze completely will not solve it in 'no time.' Hence as learning progresses the time scores tend to bunch at the lower end of the scale. Secondly, the continuous dropping out of the slowest cases in Problems 3, 4, and 5, and the fact that all who required more than 15 min. were scored equally at 900 sec. reduces the averages. The correlation of intelligence score and average time for all five problems (using only the 36 cases in which the series was completed and omitting all 900-sec. scores), gave -0.39 ± 0.09. It is evident that the group of 36 is highly selected and made up of better records only. If all of the 67 *Ss* had been allowed to complete all of the problems the correlation would probably have been higher.

Intelligence Scores. In order to bring out contrasts among different intelligence levels in time scores, cues and imagery, rational or logical procedure, the *Ss* have been divided into three intelligence groups as follows:

Group I. *Ss* with Thorndike scores more than 1 S.D. above the mean (14 cases, av. score 96.64 ± 6.10). See Fig. 1 for individual time scores.

Group II. *Ss* with intelligence scores between 1 S.D. and -1 S.D. (41 cases, av. score 70.50 ± 7.62). See Fig. 1 for the time scores of 12 cases from this group, selected at random.

Group III. *Ss* with intelligence scores more than 1 S.D. below the mean. (12 cases, av. score 48.45 ± 4.8). See Fig. 1 for individual time scores.

Time scores. The figures in Table III show that the three ability groups average in their time scores in each of the five maze problems in the same order as they register in the Thorndike test (see also Fig. 2). It may be observed, however, that the order of difficulty of the problems for the three intelligence groups is not the same. Group I follows what is probably the actual order of difficulty if the learning factor were eliminated (1,2,4,3,5). Group II shows two modifications of this order. Problem 2 is on an average not as intelligently attacked by the *S*s of average intelligence as by Group I, and the time required is therefore longer. The same is true of the procedure of Group II in Problem 3. By the time the *S*s reach Problem 4 they have

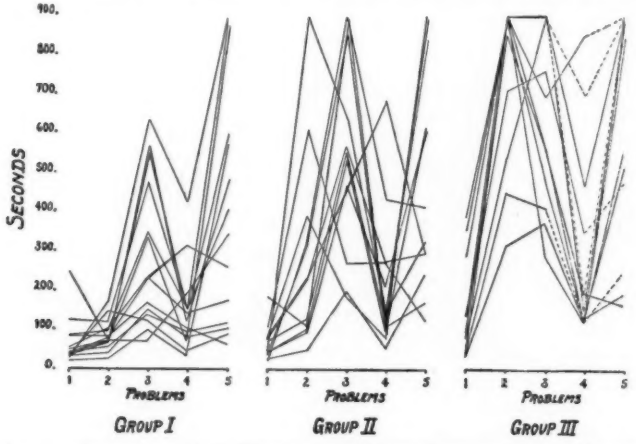

FIG. 1. INDIVIDUAL TIME RECORDS ON MAZE POSITIONS FROM THREE COLLEGE INTELLIGENCE GROUPS. GROUP I, SUPERIOR, GROUP II AVERAGE, GROUP III INFERIOR INTELLIGENCE

spent almost as much time (921 sec.) as Group I spends on the entire series (1013 sec.); the maze pattern by this time is to some extent *learned* and so Problem 4, which is really harder than Problem 2, is solved in less time. Similarly, Problem 5 is solved in less time than Problem 3, which is undoubtedly harder. Group III shows the order of difficulty even more disturbed by the learning factor introduced by slowness (1,4,5,3,2). Work on the next to the easiest problem—Problem 2—gives these *S*s so much experience with the maze pattern that the far harder Problem 3 is more quickly solved than Problem 2; the hardest problem of all—Problem 5—is done in

less time than the two easier problems (Problems 2 and 3); and Problem 4, which is harder than Problem 2 requires on an average less than one third as long for solution.

In spite of the agreement between intelligence group placement and time score the variability of individual scores is large within each of the three groups (see Fig. 3). The probable correctness of the difference between the averages of the groups has been computed by Boring's method and formula to indicate the 'correctness' of the means.[10] The difference between each two means so measured is greater than six times the probable error of the means. This gives an index of 1.00 for the probable correctness of the position of the means of the three groups, *i.e.* they are 'certain.'

TABLE III
AVERAGE TIME SCORES (IN SEC.) ON 5 SPACE PROBLEMS FOR THREE INTELLIGENCE GROUPS

Intelligence Group	Problem				
	1	2	3	4	5
I	55	84 (±7)	318	137	419
II	94	338 (±29)	489	237	431
III	138	763 (±39)	595	255	462
Av. total no. cases	96	361	463	217	432

The degree to which each group overlaps the median of another has also been determined for one of the problems (Problem 2) by Kelley's method[11] of measuring overlapping in which the reliability of the measure is taken into account. We find an index of 1.00 in five of the six comparisons; Group III does not overlap the median of Group II or Group I, nor is the converse true; Group I does not overlap the median of Group II. Eight percent, however, of the cases in Group II overlap the median of Group I. The percentage of each group whose time score exceeds a given number of seconds is given graphically in Fig. 3.

TABLE IV
SPEED OF MOVEMENT AT THREE LEVELS OF INTELLIGENCE

Intelligence Groups	Rapid movers		Normal or slow movers	
	%	No.	%	No.
I	50	7	50	7
II	42	18	58	23
III	24	3	76	9
Total	42	28	58	39

The time scores indicate speed or slowness in reaching the exit. Speed of movement has been distinguished from speed as registered in time score. Those who move rapidly have been differentiated from the group of normal and (a few cases of) subnormal speed. The results in Table IV appear for the three intelligence groups.

[10] E. G. Boring, On the computation of probable correctness of differences, this JOURNAL, 28, 1917, 454-459.
[11] T. L. Kelley, Measurement of overlapping, *J. Educ. Psychol.* 10, 1919, 229-232.

Thirty-nine *Ss* did not move faster than a fairly moderate rate. About 5 of these might have been classified as slow movers; sluggish in every motion, making long pauses between moves. The most intelligent group was divided equally between the normal and the rapid rate. Group II showed the same proportion of fast workers as the group as a whole, *i.e.* just less than half were fast. Of the dull group one quarter were fast. One of these was probably the quickest of all the 67 *Ss* as far as motion alone was concerned, but the motions were trial and error with very little evidence of learning by experience.

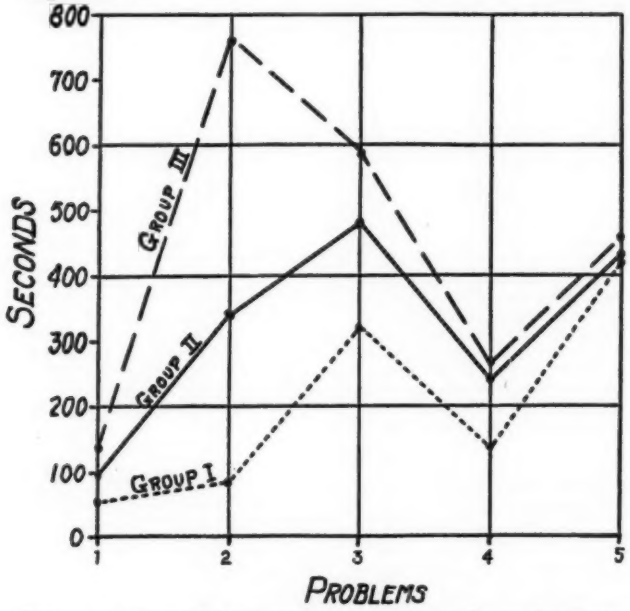

FIG. 2. AVERAGE TIME RECORDS ON 5 MOTOR SPACE PROBLEMS FOR THREE INTELLIGENCE GROUPS

Error Scores. The amount of error is significant in connection with the time score. In comparing the three intelligence groups with respect to errors the following classification into error groups has been used: 0 is the index of performance freest from error; 1 means some error largely confined to one part of the maze; 2 means some error in various parts of the maze; 3 means much error throughout. Errors could and did

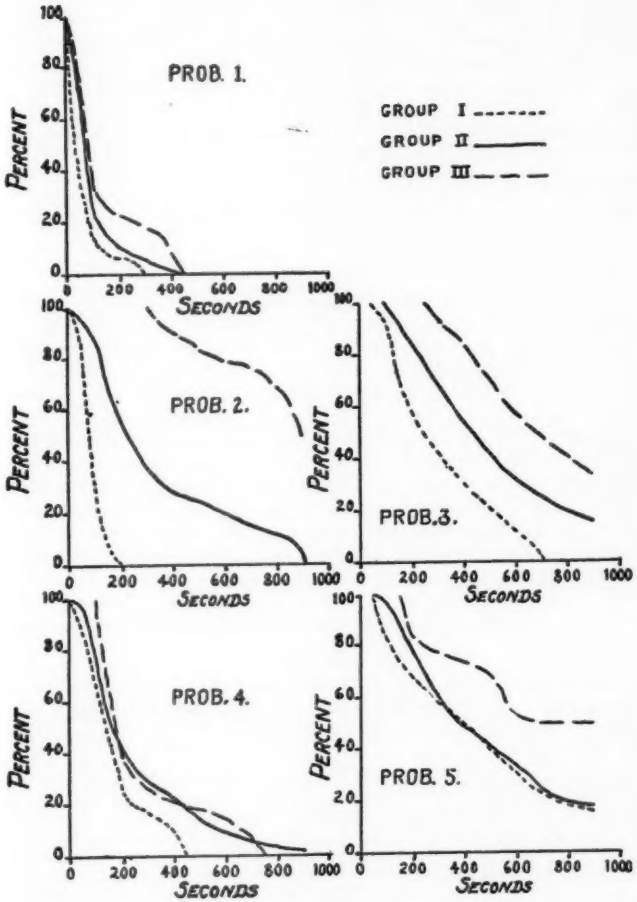

FIG. 3. GROUP TIME RECORDS ON 5 MAZE POSITIONS. THE PERCENTAGE OF EACH INTELLIGENCE GROUP WHOSE SPEED EXCEEDS ANY GIVEN NUMBER OF SECONDS

result even when the *Ss* were using intelligent plans of procedure. These ratings are given in Table V, and are, of course, subject to the constant error of a single judgment and the unreliability of results based on small groups. Still they are as obviously indicative of a general trend.

TABLE V

ERRORS IN MAZE PROBLEMS AT THREE LEVELS OF INTELLIGENCE

Intelligence Groups	Error Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	14	2	29	4	36	5	21	3
II	55	23	33	13	10	4	2	1
III	83	10	17	2	0	0	0	0

Group I has 21% of its members in the class practically free from error, 36% in the class of few errors, 29% in the class of many errors, and is represented by 2 cases—14%—in the worst class of all. Group II has only 2% in the class free from error, 10% in the slight error class, 33% in the many error class, and 55%—more than half—of all its cases in the worst class of all. Group III has no cases in the first two classes, almost 17% in the third class, and the remaining, 83%, are in the worst class. Some individuals scoring highest in intelligence score among the poorest in number of maze errors, but of those who score low in intelligence there were in the present experiment none whose performance was as free from error as 57% of the superior and 15% of the average *Ss*.

V. INTELLIGENCE AND METHODS OF PROCEDURE, IMAGERY, AND OTHER CUES

Intelligence and trial-and-error procedure. The extent to which trial and error governs procedure has been rated for each individual performance in the series of experiments (see Table VI).

Degree of incoördination (the extent to which the two hands operated independently in time, but more especially in direction), repetition of the same errors, failure to push toward (the outside when other schemes fail, and the extent of occurence of random, apparently unreasoned movement, have been considered in making the rating. Index 3 means that behavior was best described simply as "trial and error;" Index 2, lapse to trial and error at least half of the time; Index 1, occasional lapses to trial and error; Index 0, absence of trial and error, evidence that behavior was rationally motivated in all of the problems.

Two cases of Group I fell into trial and error occasionally, and 2 cases at least half of the time, the 10 remaining cases (72%) appeared to show that behavior was reasoned throughout. Group II had one case free from trial and error (2%), 14

cases (35%) who lapsed occasionally, 17 cases (41%) whose performance was at least half of the time of the trial-and-error type, and 9 cases (22%) trial and error throughout. Group III had no cases free from trial and error, one case (8%) almost free, 5 cases (42%) at least half trial and error, and 6 cases (50%) whose performance was trial and error throughout.

TABLE VI

TRIAL AND ERROR AT THREE INTELLIGENCE LEVELS

Intelligence Groups	Trial-and-Error Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	0	0	14	2	14	2	72	10
II	22	9	41	17	35	14	2	1
III	50	6	42	5	08	1	0	0

Intelligence and imagery. All of the *S*s in all three Thorndike groups (with one possible exception, a member of intelligence Group III) made use of visual guidance in solving the maze problems. Similarly, kinaesthetic guidance was apparently in every case an element in solution. The typical situation was that in which the *S* 'looked at' the upper maze and 'felt' the lower maze. In Table VII each individual has been arbitrarily assigned to one imagery classification or another, on the basis of *his statement* of the type of imagery that prevailed in his case. Perhaps a method of rating the *extent* to which each type of imagery was used would have given a more adequate characterization of procedure. The more intelligent *S*s probably did depend on imagery to a greater *extent* and *more effectively* than the less intelligent.

TABLE VII

PREDOMINATING TYPE OF IMAGERY REPORTED AT THE THREE LEVELS

Intelligence Groups	Visual		Kinaesthetic		Mixed	
	%	No.	%	No.	%	No.
I	14	2	50	7	36	5
II	32	13 (1 doubtful)	46	19 (2 doubtful)	22	9
III	25	3	33	4 (1 doubtful)	42	5
Total	27	18	45	30	28	19

Of the entire group of 67 almost half (45% were more aware of 'feeling' their way than of 'seeing' the path, 18 (27%) were more conscious of the visual guidance, and 19 (28%) thought 'feeling' and 'seeing' were equally important to them. The more intelligent *S*s (Group I) were more apt to consider the kinaesthetic imagery as more important (50% kinaesthetic);

the average *S*s (Group II) 19 cases (2 doubtful) (46%) believed they had depended on kinaesthetic imagery; and 4 cases (1 doubtful) (33%) of the inferior group (Group III) were conscious of kinaesthetic aids.

The difference between the superior and the inferior group in kinaesthetic guidance seems significant, and agrees with the observations of the extent of trial-and-error performance in these groups. The fact that only 14% (2 cases) of the high testing individuals, 32% (13 cases, one doubtful) of the average (Group II), and 25% (3 cases) of the inferior group (Group III) thought of the predominance of visual imagery seemed at first surprising. It may perhaps be explained by the greater attention of the more intelligent individuals to a less usual aspect of experience (the kinaesthetic) because it proved to add to the visual cues and imagery, which were mentioned by all, a fairly dependable guide in the double maze situation.

TABLE VIII

USE OF THE STARTING KEY AT THE THREE LEVELS

Intelligence Groups	Percent of group who used key		disregarded key	
	%	No.	%	No.
I	56	8	44	6
II	47	19	53	22
III	17	2	83	10
Total	44	29	56	38

In connection with visual guidance mention should be made of the use of the 'key.'[12] Effective application of this visual guide probably depended to a considerable extent on the ability of the individual to make a rational or systematic plan for procedure to which this was the one and only cue. Twenty-nine *S*s—just less than half the total group—were observed to use the 'key' as a guide or stated that they had used it. Their distribution in the three Thorndike groups is given in Table VIII. These made up more than half of Group I, about half of Group II, and a sixth of Group III. Here the two higher testing groups do not differ greatly, but the low testing individuals seem far less apt to avail themselves of this device.

(*a*) *Introspective reports.* In *E*'s notes were recorded, throughout the series, the types of procedure followed. The *S*'s introspective report given at the close of the experiment usually included mention of fewer cues and methods than had probably actually been followed. Sometimes it included those that had not demonstrably affected the procedure at all. Where the guidance of a certain principle was definitely stated, and if the effort to follow it had been obviously present even if only in part, it was credited to the individual and to the group.

[12]The 'key' is a small replica of the central part of the maze. It is fastened to a small round disk attached to the top of the maze box (see Plate I). Rotated to the position of the center of the lower maze in each problem position it could serve as a cue to the starting direction. The key could not, however, be turned over on its face.

(*b*) *Reasoned methods of maze problem solution.* In addition to the general visual-motor activity and imagery, characteristics of the response-behavior included rational plans containing specific cues, and abstract theories of procedure. Considerable disparity was shown in the *extent* to which plans were used. The behavior of the *S* and the corroboration or contradiction of his introspective account are considered in the ratings on the degree to which a theory or logical scheme directed movement. In Table IX, Index 3 means a definite guiding plan evolved and, in the main, adhered to; Index 2 means evidence of a plan functioning more than half of the time; Index 1 means evidence of a plan whose operation could at times be discerned in the *S*'s movements in the maze; Index 0, no logical plan in evidence at any time.

TABLE IX

FORESIGHT AND PLAN

Intelligence Groups	Plan Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	86	12	7	1	7	1	0	0
II	10	4	41	17	41	17	8	3
III	0	0	17	2	75	9	8	1

Of Group I, 86% (12 cases) gave evidence of a definite plan, in the main operative. No case in this group was entirely planless. Of Group II 10% (4 cases) appeared to have a definite plan and 8% (3 cases) were apparently planless. Half of the remaining cases in the group had a plan that functioned half of the time; half appeared to attempt to follow a plan, but with less success. Group III had no case with a definite plan consistently operative. Two cases (17%) had plans that functioned half of the time. Nine cases (75%) had plans that were at times discernible in action and one case that showed no evidence of plan. 'Planfulness' and Thorndike score appeared to show some agreement in the group comparisons; there were no cases in Group III as planful as those in Group I, and none in Group I that were planless.

Intelligence and other cues. (*A*) *Concrete plan elements.* Some of the specific concrete elements involved in the schemes could be separately recognized and may be so discussed. These include (1) the general notion of movement toward the outside of the maze; (2) the use of excessive pressure on the outer edges of the alleys; (3) methods for recognizing and escaping from the complication at opening 5.[13]

[13]Opening 5 is the only passage through which the true path leads inward instead of toward the outside of the maze.

(1) *The use of the cue of direction of movement toward the outside.* In the present series of experiments the object as stated is to "get out," that is, to reach the exit. Some *Ss*, even among those who plan their procedure with foresight, seem never to happen on the scheme of pushing along outside edges rather than inside in seeking alley outlets. Other individuals would succeed in getting out by use of this principle of direction alone were it not for the complication at opening 5. Even so, the chances are in favor of correction of the error at 5, for the *Ss* who direct their movements outward are quickest to discover their error, if the equal probability carries them, after they have passed 5 on the wrong side, back through 4. They discover their error because the path lengths are, of course, shorter as the center is approached.

TABLE X

INTELLIGENCE AND MOVEMENT TOWARD THE OUTSIDE OF THE MAZE

Intelligence Groups	Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	7	1	79	11	14	2	0	0
II	10	4	45	19	25	10	20	8
III	0	0	25	3	35	4	40	5

The *Ss* have been classified (Table X) into four groups according to the extent to which they use the principle of movement toward the outside. Those who use the principle consistently as a basis for solving the problem are given the Index 3. Those who use it at least half of the time are rated 2; those who use it occasionally when other methods fail are rated 1; those who use it not at all are rated 0.

Group I predominated in the percentage use of this method, Group II was second, and Group III third. In each index class the order of the groups was preserved except in the first where Group I with 1 case (7%) is exceeded by Group II with 4 cases (10%).

TABLE XI

Intelligence Group	Excessive pressure		No excessive pressure	
	%	No.	%	No.
I	14	2	86	12
II	12	5	88	36
III	33	4	67	8

(2) *The use of excessive pressure toward the outside.* A number of *Ss* who had the notion of moving the stylus along the outer edge of the paths seeking outlets were not content simply with feeling their way by keeping close along the wall. So intent were they on missing no gate that they pushed with tremendous force, at times actually pressing the maze walls out of position. The percentage distribution of those from the three intelligence groups who used excessive pressure is shown in Table XI. The two upper groups (I and II) appeared less apt (14% and 12% respectively) to use excessive pressure, than the inferior intelligence group (33%). The method appeared to be in the nature of an overdoing of the previous method.

(3) *The degree to which the contradictory character of opening 5 was recognized, and the recognition acted upon.* *Ss* who used the indirect visual guidance of the upper maze with comprehension, noticed the inward passage at opening 5, and tried to direct their movements accordingly. Only one individual succeeded in consistent avoidance of error from start to

finish. Index 3, in Table XII, means recognition of opening 5 in the first experiment and consistent avoidance of error at that point. Index 2, recognition of the fact that an inward passage existed and effort made to overcome the difficulty when met. Index 1, recognition of the nature of a recurring difficulty and escape from it with difficulty, but apparently as the result of a scheme. Index 0, no apparent awareness of opening 5; escape finally due to trial and error. The intelligence groups, in the main, fall in their order of brightness in distribution on the basis of awareness of the recurring difficulty. Group I shows the largest percentage of conscious recognition of this factor, Group III the smallest.

TABLE XII

RECOGNITION OF THE INWARD PASSAGE AT OPENING 5

Intelligence Groups	Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	7	1	49	7	22	3	22	3
II	0	0	22	9	29	12	49	20
III	0	0	8	1	0	0	92	11

(*B*) *Abstract plan elements.* In most of the performances a type of procedure was observed, at least part of the time, that indicated logical and reasoned efforts to work the space problems systematically on the basis of a thought-out plan. This procedure appeared in the form of coördination of movement between the two hands. From the *Ss*' own reports it appeared that the observed coördination was the result of any one of three theories of procedure. (1) A theory that the moving of the two hands simultaneously would offer a basis for checking on position; (2) a theory that moving the hands in response to certain verbal formulas should achieve the desired success (such formulas were "upper hand backward, lower hand forward," or "hands at right angles and outward"); (3) a direction of attention to the relation of the lines but without definite formulation of 'direction.' In many cases, as later appeared, the coördinated movement was a combined result of two or all of these tendencies.

(*1*) *Coördination of movement of the two hands.* Eleven *Ss* moved the two hands simultaneously and in the corresponding paths with only slight occasional slips. Nineteen *Ss* showed coördination most of the time. Twenty *Ss* showed some effort to coördination at the beginning of each problem, but later lapsed into independent movements of the two hands. One *S* completely dissociated the performance in the two mazes by tracing completely the upper maze before attempting the lower one at all. Fourteen *Ss* moved the two hands at the same time but without correlating direction or amount of path covered. These 14 and the one unique case are rated as showing no coördination. In Table XIII, Index 3 means definite and precise coördination, practically throughout; Index 2, tendency to coördinate with occasional lapses followed by recovery of coördination or successful exit from the maze; Index 1, partial coördination, lapses not followed by recovery, tendency to coördinate movement in a certain area, but not in every movement; Index 0, no coördination — hands moved

quite independently of one another. In each index class the three groups follow in percentage of coördination in the order of their intelligence. Group I uses coördination to the greatest extent; Group III to the least.

(2) *Other cues and plan elements.* In the introspective reports a miscellaneous group of cues were mentioned. Some of them were elements included in the plans discussed above, while others were more inclusive than our classifications and have been split up into the parts analyzed in the previous paragraphs. The results of two comparisons seem significant (Table XIV A and B): (1) the *total number* and average number of cues reported by the *Ss* in each of the three intelligence groups; (2) extent to which a plan was made up of predominantly concrete or abstract elements. The possible discrepancy between cues reported and cues actually followed should be kept in mind particularly in the case of the report of 'relations of direction' as a method of solution. A number of *Ss*, notably from the two lower Thorndike groups, who stated that such relations served as a cue, if judged by their performance, must have had a very vague notion, if any, of the actual relation of directions in the two mazes.

TABLE XIII

COÖRDINATION BETWEEN THE TWO HANDS

Intelligence Groups	Index							
	3		2		1		0	
	%	No.	%	No.	%	No.	%	No.
I	35	5	50	7	15	2	0	0
II	15	6	25	11	25	11	35	11
III	0	0	8	1	55	7	36	4

The figures in Table XIV A and B, showing the *Ss'* remembrance of the cues that had seemed most important, or of which they had been most conscious, do not necessarily agree with the figures in Tables III–XIII, which were based upon *E's* notes recorded during the progress of the problem solving. The *Ss* mentioned three concrete methods of solution: (1) direction of movement toward the supposed position of the exit; (2) direction of movement away from the position, or the supposed position, of the center; and (3) pressure toward the outside (*i.e.* on the outer edges of the path). Totaling the number of cases from each group who reported using these definite schemes we have (Table XIV A) 29% from Group I, and 73 and 67% from Groups II and III, respectively. The difference between the last two groups is small, perhaps insignificant, but both report a significantly larger number of method elements of this kind than the members of Group I. In Table XIV B are given the same plan elements expressed as percentages of total number of cues reported. The concrete cues made up only 14% of all the cues reported by the superior group; 39% for Group II, and 44% for Group III. Group I has not only reported fewer concrete cues, but these are also a far smaller percentage of the total cues reported than is the case in the other two groups.

Three abstract plan elements were also reported: (1) The method of coördinating the movement of the two hands; (2) the method of giving oneself verbal directions as to hand movements; and (3) the method of observing the relation of the two hands in the maze. Each of these methods involves a more complicated order of procedure than the simple practical measures discussed above. The latter are directed simply toward the solving of the out-of-view problem, the former are based upon the double-maze situation and are directed to a solution of it *in relation* to the visible maze. In the case of the three abstract elements of methods the

TABLE XIV

INTELLIGENCE AND METHODS OF MAZE SOLUTION

Part	Intelligence Group	Concrete (Specific) Plan Elements								Abstract (General) Plan Elements								Total concrete and abstract	
		Direction of movement toward the supposed position of exit		Direction of movement from the supposed position of center		Pressure toward outside		Total methods involving concrete (specific) elements		Coördination		Verbal directions		Relation of line in two mazes		Total methods involving abstract (general) elements			
		%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.
A*	I	0	0	14	2	14	2	29	4	71	10	21	3	86	12	178	25	207	29
	II	27	11	2	1	44	18	73	30	20	8	17	7	76	31	112	46	185	76
	III	42	5	8	1	17	2	67	8	33	4	0	0	50	6	83	10	150	18
B†	I	0	0	7	2	7	2	14	4	34	10	10	3	41	12	86	25	100	29
	II	14	11	1	1	24	18	39	30	11	8	9	7	41	31	61	46	100	76
	III	29	5	6	1	11	2	44	8	22	4	0	0	33	6	56	10	100	18

*Gives the percent of individuals in the group who reported a certain cue. Since one individual may report more than one cue, the average of this row may be more than 1.00.

†Gives the frequencies of cues expressed as percentages of the total cues for the group.

general order of percentages shows Group I with 1.78%, Group II, 1.12%, and Group III, 0.83%. (The group percentages may be more than 1.00, because one individual may report more than one plan element.) These respective plan elements are 86, 61 and 56% of the total procedure types reported. Here the order is reversed from that in the case of the concrete simple plan elements.

The totals of both sorts of plans show Group I with 2.07% of plan elements, Group II with 1.85%, and Group III with 1.50%.

The group of superior intelligence (Group I) has one third more plan elements in conscious remembrance than Group III, and more than twice the number of abstract plans. Group II falls about half way between the superior and the inferior groups.

VI. Conclusions

(*1*) In solving motor space problems on the two-story maze, success (measured by time scores) and intelligence (measured by scores on the Thorndike College Entrance Test) showed a significant correlation. In three out of five problems the correlation coefficient was more than four times the probable error. In the two problems where the correlation was not significant, learning had entered into the situation, so that the problem was for many of the subjects no longer a new experience. On the series as a whole (5 problems) the correlation of speed with Thorndike score was also significant.

(*2*) Differences in reaction pattern appeared between three intelligence groups.

(A) Group I (14 cases) (Thorndike Intelligence score 88.5–110). These *Ss* (1) made a shorter average time record, (2) fewer errors, (3) used trial-and-error less frequently, (4) used the 'key' as a guide more often, (5) recognized a contradictory element in the maze and overcame the difficulty more frequently, (6) definitely coördinated their hand movements, (7) moved in the general direction, but (8) did not make extensive use of excessive pressure, (9) were quicker in movement, (10) showed far greater evidence of foresight and planfulness, (11) relied slightly more on kinaesthetic cues, and (12) used more cues and tended toward the abstract and general in schemes of solution.

(B) Group II (41 cases) (Thorndike score 55.50–88.50) is intermediate between Groups I and III in the points in which the superiority of Group I was noted, except possibly with respect to excessive pressure. Groups I and II are clearly differentiated from Group III though not from each other in regard to pressure. *Ss* of Group II reported more conscious dependence on visual imagery than did either of the other groups. In number of cues used, Group II is nearer Group I than Group III.

(C) Group III (12 cases) (Thorndike score 35.0–55.50) is inferior to both other groups on every count except excessive pressure where its members exceed both other groups.

(D) The groups are decidedly variable and there is much overlapping of performance.

(*3*) Practically all of the *S*s reported visual and kinaesthetic guidance or cues. Members of Group I were more apt (50%) to be conscious of the value of kinaesthetic imagery, than of mixed (36%), or especially of visual (14%) in this performance. Group II also believed themselves more dependent on kinaesthetic imagery (46%) but a considerable number of them reported visual imagery (32%) and mixed imagery (22%). Group III reported mixed imagery most frequently (42%), then kinaesthetic (33%), and visual (25%). The typical procedure was in all the groups to 'look at' the upper maze and 'feel' the lower one, and imagery in most cases aided these efforts to make as coördinated a performance as was achieved.

(*4*) For the solving of maze problems presenting new space relations intelligence as measured by Thorndike score seems to be an asset. The test index accords with speed of movement and with freedom from error and it seems also to register the ability of individuals to make use of imagery effectively, to organize logical and efficient plans of procedure, and to 'see through' (or 'feel through') the relations between a visible space situation and a similar one out-of-view.

(*5*) In the series of 5 problems one was found that appeared, when preceded by a practice problem, to offer a favorable performance test of intelligence. The correlation between time scores on Problem 2 in this series and Thorndike score was high ($+0.59 \pm 0.06$). With a wider intelligence range than a college group offers it might be standardized into a useful discriminative test.

FALSE SUGGESTION AND THE PIDERIT MODEL

By Samuel W. Fernberger, University of Pennsylvania

The following is the fourth report of a series of experiments performed at the University of Pennsylvania with the Piderit model for the demonstration of facial expression.[1]

Some years ago Buzby[2] showed six faces (Anger, Dismayed, Horrified, Disdainful, Disgusted and Bewildered) to 716 *Ss*. These *Ss* were merely asked to choose the best descriptive term from a list of eighteen among which were six correct terms. The results show a wide scatter of judgments and the percentages of correct judgments are relatively small.

In spite of these findings it is evident that the Piderit model is a very useful instrument for class demonstration. Jarden and Fernberger[3] showed the same six faces to 995 *Ss* with two different degrees of suggestion. The faces were shown and the emotion was named and the *Ss* were asked to judge (1) if the face was not a representation of the emotion, (2) if it was a poor, (3) a fair, or (4) a good representation of the emotion named. Except in the case of Anger, the percentage of correct judgments was very great—80% or over and, in one case, as high as 99%. Several months later, the same faces were shown to the same *Ss* with a very high degree of suggestion—the expression was analyzed and the face built up before the group. In the case of Anger there was a still further increase of correct judgments. For those faces which already had a very high percentage of correct judgments with the lower degree of suggestion, these percentages were not further increased.

Fernberger[4] worked all three procedures on six more faces—Quizzical, Amazed, Contemptuous, Attentive, Stubborn, and Reverential—with 750 *Ss* and obtained results which were exactly comparable to those of Buzby, and of Jarden and Fernberger. He found a wider scatter when no suggestion was given, a high percentage of correct judgments when the lower degree of suggestion was given, and a somewhat but not considerably higher percentage with the complete suggestion of analyzing and building up the faces.

Procedures. In the present experiment, we employed the same six faces used by Buzby, and by Jarden and Fernberger, viz., Anger, Dismayed, Horrified, Disdain, Disgusted, and Bewildered. Exactly the same procedures were again used.[5]

Subjects. For the first procedure (picking the best descriptive term from a list of 18) 666 *Ss* were used; for the second procedure (face 'named') 658 *Ss*; and for the third procedure (face 'analyzed') 630 *Ss*. None of these had been *Ss* with Buzby or with Jarden and Fernberger.

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[1] E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, this Journal, 34, 1923, 471-485.

[2] D. E. Busby, The interpretation of facial expression, *ibid.*, 35, 1924, 602-604.

[3] E. Jarden and S. W. Fernberger, The effect of suggestion on judgment of facial expression of emotion, *ibid.*, 37, 1926, 565-570.

[4] Fernberger, Six more Piderit faces, *ibid.*, 39, 1927, 162-166.

[5] For a complete description of instruction, cf. Jarden and Fernberger, *op. cit.*, 565-566.

Results of first procedure. The results for the first procedure will be found in Table I. The number of judgments for each of the 18 descriptive terms appear in successive columns. The number of judgments for the correct terms are in boldface. The results show the usual scatter—for Anger and Dismayed every one of the 18 terms was checked at least three times; for Horrified and Disdain all but 2 of the terms were checked; for Bewildered all but 4 and for Disgusted all but 7 terms were checked. Although the percentages are not identical, the scatters are a very close confirmation of the Buzby results with these same faces. Indeed the rank order of correct judgments is exactly the same in both cases: Horrified and Disgusted are highest; Disdain and Bewildered intermediate; and Dismayed and Anger very low.

TABLE I

FREQUENCIES OF JUDGMENT WITHOUT SUGGESTION

666 cases	Anger	Dismayed	Horrified	Disdain	Disgusted	Bewildered
Pleased	155	4	6	2	—	2
Displeased	6	12	4	32	13	1
Stubborn	4	15	3	69	7	4
Attentive	7	294	—	4	—	103
Quizzical	69	114	2	11	—	115
Inattentive	5	11	3	35	—	24
Dismayed	21	28	22	15	1	32
Reverential	6	38	—	21	1	40
Affable	60	10	4	2	—	18
Bewildered	67	51	24	7	1	184
Amazed	26	17	55	—	—	136
Horrified	23	3	355	—	2	1
Raging	25	3	65	2	1	—
Disapproving	15	28	13	92	54	5
Disdainful	57	19	10	210	73	1
Anger	15	3	9	3	—	—
Contemptuous	79	8	26	115	226	—
Disgusted	26	8	65	46	287	—

From the former experiments it is evident that there was a great effect of suggestion and we decided to see how well one could 'get across' false suggestion by the two procedures of 'naming' and 'analyzing' the faces.

It was decided to use in the second and third procedures, three terms in the same emotional series (differing only in degree), and three terms in different emotional series. For the same emotional series we called the Dismayed face, 'Quizzical;' the Disgusted face, 'Contemptuous;' and the Disdain face, 'Displeased.' For the different emotional series we called the Anger face, 'Pleased;' the Horrified face, 'Attentive;' and the Bewildered face, 'Reverential.' The true faces and the false suggestions for the two emotional series are tabulated below:

Same Emotional Series		Different Emotional Series	
True Face	*False Suggestion*	*True Face*	*False Suggestion*
Dismayed	Quizzical	Anger	Pleased
Disgusted	Contemptuous	Horrified	Attentive
Disdain	Displeased	Bewildered	Reverential

The terms we chose had varying degrees of validity of judgment in the original scatter. The Anger-Pleased, Dismayed-Quizzical, and Disgusted-Contemptuous combinations had high validity, and the Horrified-Attentive, Disdain-Displeased, and Bewildered-Reverential combinations had low validity. Indeed, Attentive was one of the two terms not checked at all for the Horrified face.

Results of second and third procedures. The results of the judgments under the two degrees of false suggestion are given in Table II. Under the caption 'named' are given the percentages of 'good,' 'fair,' 'poor,' and 'no' judgments for each of the six

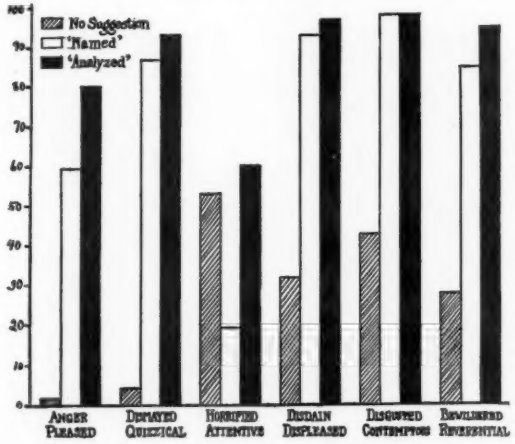

Fig. 1. Comparison of Results

faces for the lower degree of false suggestion. Under the caption 'analyzed' are given the percentages for the higher degree of false suggestion. The accompanying chart (Fig. 1) gives the percentages of 'correct' judgments under all three instructions. The shaded portions indicate the percentages of correct judgments with no suggestion, *i.e.* the percentages of the actually correct name of the face. The white portions indicate the percentages of 'correct' judgments under the 'named' degree of suggestion, *i.e.* the percentages of the 'good,' 'fair' and 'poor' judgments combined. The black portions indicate similarly the

TABLE II

RELATIVE FREQUENCIES OF JUDGMENT WITH TWO DEGREES OF FALSE SUGGESTION

True Face	False suggestion	'Named'—658 cases				'Analyzed'—630 cases			
		Good	Fair	Poor	No	Good	Fair	Poor	No
Pleased	Anger	6	24	29	41	16	33	31	20
Quizzical	Dismayed	31	43	13	13	47	36	10	7
Attentive	Horrified	5	5	9	81	11	20	30	40
Displeased	Disdain	39	38	16	7	61	29	7	3
Contemptuous	Disgusted	83	12	3	2	80	16	3	2
Reverential	Bewildered	29	41	15	15	45	37	13	5

combined percentages of 'correct' judgments for the 'analyzed' degree of false suggestion.

The results for four of the faces (Quizzical-Dismayed, Displeased-Disdain, Contemptuous-Disgusted, and Reverential-Bewildered) show a similar distribution differing only in degree. In each of these four cases the percentage of correct judgments is relatively low when no suggestion is given. These percentages increase enormously with the lower degree of false suggestion and, except in the case of Contemptuous-Disgusted, increase slightly again for the higher degree of false suggestion. The results for Contemptuous-Disgusted indicate that both degrees of false suggestion carried in 98% of the judgments. The higher degree of suggestion was adequate to produce a false judgment, in these four cases, in at least 93% of the cases. The lower degree of suggestion was adequate to produce a false judgment in these four cases in at least 85% of the *Ss*.

It will also be observed in Table II that, in the cases of Dismayed-Quizzical, Displeased-Disdain, and Reverential-Bewildered, the higher degree of suggestion gives a higher percentage of judgments in the 'good' and 'fair' categories.

The results for Attentive-Horrified are quite different from all the others. In this case we chose a face violently contorted in expression and tried to suggest it as an emotion for which the expression is relatively calm. Also we were here dealing with a face that 'gets across' in its own right very much better than any of the others (56% correct judgments with no suggestion).

When the face was 'named' falsely, the percentage of correct judgments dropped to 19. In every other situation it was considerably increased.

We therefore found it necessary, under the 'analyzed' false instruction, to create a general situation which might prove adequate. We told the *Ss* that this was a special case of attention, such as would be apparent if an individual had been giving a very high degree of attention in one direction at the time when a compelling stimulus from another direction forced attention to it. We emphasized the importance of the adjustment of muscular mechanisms for all attention and of the general muscular tenseness for all keen attention, and finally used again an invented proverb that one attends better while breathing through the mouth than through the nose.[6] We then acted out the situation and finally built up the face.

That this suggestion was successful, in part at least, is evidenced by the fact that there were 60% of correct judgments. This percentage for Attentive-Horrified, under the highest degree of suggestion, is considerably lower than that for any of the other five faces. Still it is reasonably adequate when one considers that the false suggestion is 9% better than when there is no suggestion at all.

Three of the terms employed in the present experiment are repeated from Fernberger's former study where they were used as true suggestions, while in this study they are used as false suggestions. It is of interest to note that the false suggestion is actually better than the true. The results for Contemptuous are practically the same for both degrees of suggestion and very high in both cases. Those for Quizzical and Reverential are better for false than true suggestion for both degrees. This means that Bewildered was judged 'correctly' more times as Reverential than Reverential was itself. Similarly Dismayed was judged 'correctly' more times as Quizzical than was the correct Quizzical face. This result was probably due to the greater enthusiasm of the experimenter in trying to 'sell' or 'put across' the faces in false suggestion than in the true.

The results for both degrees of false suggestion were analyzed to determine a possible significant sex-difference in suggestibility. In order to save space, this analysis is not included inasmuch as the results were entirely negative. No significant sex-differences in suggestibility of either the lesser or greater degrees of false suggestion were evident.

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[6]We were not able to find out for how many *Ss* this invented proverb had a familiar ring. After telling some of the later groups how the results were coming out and what we had been trying to do, many of the *Ss* volunteered the information that they had thought the proverb an old and familiar one.

It would seem that the results outlined above have a certain theoretical significance. They would indicate that little can be determined of an emotional state in another individual from the perception of an abstracted facial expression completely divorced from a general, total stimulus-situation. If a stimulus-situation is indicated, the emotional state is judged in accordance with that situation rather than in accordance with the facial expression. This judgment is made correctly with a high probability if the stimulus-situation is correctly indicated (Jarden and Fernberger) and it is made incorrectly with quite as high a degree of probability if the stimulus-situation is falsely indicated. Indeed, in the cases of Pleased-Anger and Quizzical-Dismayed, the false suggestion gave a higher percentage of 'correct' judgments than the true suggestion had formerly. This would indicate that our perception of the emotional states in others are in the nature of social meanings dependent more upon the stimulus-situation than anything else.

This point of view is also indicated in the results of Sherman.[7] Working with infants under 12 days of age, Sherman experimentally produced hunger, fear, anger, and pain. Motion pictures were taken of the stimulus-situation and of the reaction. These were shown for (1) reaction only without the stimulus-situation, (2) reaction with the correct stimulus-situation, and (3) reaction with a transposed or false stimulus-situation. Graduate students in psychology, medical students, student nurses, and Normal School freshmen acted as *Ss*. The results show a wide scatter of judgment when the reaction is shown alone, much greater validity of judgment when the stimulus-situation and the reaction were both shown, and also a greater but false validity with the transposed stimulus-situations. Sherman concluded in part: "The results indicate that the stimulus preceding the reaction shown was usually the deciding factor in the name given to the reaction."[8]

It is of interest to mention in passing that in analyzing the faces, one could logically find as adequate a biological reason why each of the features should be shown in the false suggestion as had been true in the true suggestion several years ago. This aspect of the problem, which involves much more extended treatment than we can give here, may be of some importance in criticizing the theories of the followers of the Darwinian school of the emotions.

Summary

(1) The results for the Piderit model, when no suggestion was given, showed a wide scatter with little validity of correct judgment in the interpretation of expression of emotion. This is a complete confirmation of the findings of Buzby working with the same six faces representing Anger, Dismayed, Horrified, Disdainful, Disgusted, and Bewildered.

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[7] M. Sherman, The differentiation of emotional responses in infants: I. Judgments of emotional responses from motion picture views and from actual observation, *J. Comp. Psychol.*, 7, 1927, 284.

[8] *Op. cit.*, 284.

(2) With the lower degree of suggestion involved in 'naming' the face, false suggestion led to a great increase in the percentage of 'correct' judgments when the false suggestion represented Anger as Pleased, Dismayed as Quizzical, Disdain as Displeased, Disgusted as Contemptuous, and Bewildered as Reverential.

(3) With the higher degree of false suggestion involved in 'analyzing' the face, a relatively great increase in 'correct' percentages was found for the Anger-Pleased situation and only a slight increase for the other four situations. All of the other four faces gave 'correct' judgments of more than 93% under this higher degree of false suggestion.

(4) Horrified gave a high percentage of correct judgments when no suggestion was given and a considerably lower percentage when the face was merely falsely named. It was found possible to create, under 'analyzed,' a situation which gave the highest percentage of correct judgments for this face (60%).

(5) These results seem to indicate that the perception of emotional states in others by their expression is of the nature of social meanings and that they are much more dependent on the stimulus-attention than anything characteristically intrinsic in the facial expression or reaction.

SENSITIVITY TO ODORS AND OTHER PHENOMENA DURING A FAST

By JOHN ARTHUR GLAZE, University of Michigan

In a previous experiment it was found that some *S*s showed a marked increase in sensitivity to odors during a fast.[1] This was particularly true with the *S*s that endured long fasts of 17 and 33 days. The observations of Marsh, as we mentioned in the earlier study, confirm these findings.[2] As neither Marsh's nor our study was designed to test this phenomenon, the following study was undertaken.

TESTS WITH ODORS

Apparatus. The Zwaardemaker olfactometer was employed in the experiments. It consists of "one tube with a curved end to be inserted in the nostrils, and of a second tube of larger size containing an inner lining of the substance to be investigated which slips over the former. The amount of stimulation is measured by the area of the outer tube exposed beyond the inner."[3] As a matter of convenience we shall call the tube containing the substance the *x* tube, and the inner conducting tube that extends through the shield to the nose the *y* tube. The end of the *y* tube was covered by a smooth nickel cap so that it closed the side of the nose in which it was placed. The *S* always closed the other nostril with a finger during the test.

Seven odoriferous tubes were used. The liquid odors were diffused through warm paraffine, poured into tube *x*, and a hole the size of tube *y* was bored through the paraffine after it had hardened. The ends of the *x* tubes were sealed with tin foil so as to hold the substances and also to prevent wear at the ends. A hole, the size of tube *y*, was of course made in the center of the tin foil caps.

Stimuli. The substances used were asafoetida, benzoin, cedar wood, rubber, balsam, beeswax, and Russian leather. The first was very weak, the last very strong, the others ranged in strength between these. When not in use the tubes were kept in screw-top glass containers.

Procedure. Several *y* tubes were used, and they were thoroughly washed and dried at the end of every test. When tube *x* was placed on tube *y* it was shoved beyond the end so that tube *y* projected out of tube *x*. *S* then blew the air out of *y* and, after a short wait, placed the nickel cap over the end of the *y* tube and the experiment began. Tube *x* was exposed slowly until *S* indicated that he scented the odor. Then the *x* tube was shifted in the other direction until *S* reported that the odor was lost. The mean of these two points was taken as the *S*'s limen. Only in a few cases were these deviations large, and these few cases were more apt to occur with the strong and weak odors than with those of intermediate strength. It took from 20–30 min. to test an *S* with all 7 odors, consequently a rest

*Accepted for publication February 29, 1928.

[1] J. A. Glaze, Psychological effects of fasting, this JOURNAL, 40, 1928, 236-253. *Cf.* especially 250 f.

[2] H. D. Marsh, Individual and sex differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 438.

[3] W. B. Pillsbury, *The Fundamentals of Psychology*, 2nd ed. 1922, 180.

period of about 5 min. was usually interpolated between the fourth and fifth tubes. Sensitivity was measured by the number of mm. of the odoriferous material exposed.

Subjects. Two *S*s were used: B, the writer, who served in the previous experiment; and D, a 10-yr. old boy, a patient in the University of Michigan Hospital. Eight weeks before the beginning of the experiment, D was admitted to the hospital and shortly afterward was operated on for tonsils and adenoids. He was able to walk from the ward to the laboratory where the tests were held.

D was up to grade in school subjects, had a mental age on the Binet test of 9½ yrs., and I.Q. of 87. He was decidedly overweight for his age and was willing to fast to reduce. Indeed, he reported that he had fasted before, and that he had gone as many as 5 days without food so as to get rid of his excess weight—a subject about which he was sensitive.

B fasted for 10 days, and D for 5 days. As in the earlier study, a practice period preceded the fasts. In general the pre-fast and post-fast test periods were of about the same length as the fast periods. Neither *S* showed signs of a cold throughout the experiment, and D never left the hospital.[4]

Table I

Sensitivity of B and D to Odors During the Pre-Fast, Fast, and Post-Fast Periods

Sensitivity expressed in mm. of odoriferous material exposed

Pre-Fast				Fast				Post-Fast			
B		D		B		D		B		B	
day	mm.	day	mm.	day	mm.	day	mm.	day	mm.	day	mm.
1	*	1	*	11	60.1	7	19.0	21	29.3	12	25.3
2	69.0	2	53.0	12	34.0	8	6.9	22	22.0	13	18.1
3	56.4	3	22.6	13	25.4	9	− 0.6	23	41.9	14	21.9
4	44.4	4	14.6	14	13.4	10	− 1.4	24	26.7	15	28.9
5	37.7	5	20.9	15	− 2.1	11	− 1.7	25	26.6	16	48.9
6	48.1	6	36.7	16	− 9.7			26	37.0		
7	63.3			17	− 3.7			27	31.6		
8	42.3			18	0.6			28	38.4		
9	70.0			19	0.7						
10	66.4			20	− 1.7						

*Practice, no data taken.

Results. The results are shown in Table I. Minus values are shown better after the second day of D's fast and after the fourth day of B's fast. This means that sensitivity was so acute that the *S*s were able to scent some of the odors before the *x* tubes were drawn out and exposed in the *y* tubes. In the case of Russian leather and part of the time in the cases of beeswax and balsam this was quite pronounced. At first *E* was suspicious that the *S*s had anticipated the odor, but when the report was verified on successive days there remined no doubt but that the first measures were accurate.

[4] I have Dr. David M. Cowie and his assistants of the University Hospital staff to thank for their kindness in allowing me to use this *S* and in giving instructions to the diet kitchen to withhold the plate from him during the 5 days.

It must be said that tube y did not completely fill tube x. Six of the y tubes were so nearly the same diameter that they were used indiscriminately with any of the 6 odors. The seventh y tube was slightly smaller than the others and was always used with the rubber x tube. Minus values mean then that S was able to scent the odors when none of the x tube projected beyond the end of the y tube. Russian leather, beeswax, and balsam had large minus values whereas asafoetida was exposed as much as 50 mm. on some days during the fast to reach the sensory threshold. Benzoin was also weak. The averages of the 7 odors, given in Table I, do not show how difficult it is to sense some of the stimuli.

Asafoetida, rubber, and benzoin were not scented at times during the pre- and post-fast periods even when they were exposed at their full strength—100 mm. We counted these

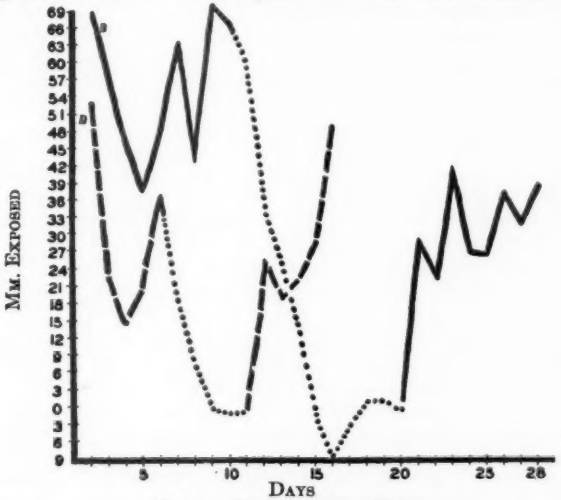

FIG. 1. SENSITIVITY TO ODORS
Broken lines show results of the fasts

cases, however, as if they were sensed at 100 mm. Ostensibly this diminishes the ratio of increased sensitivity during the fast, since we do not know how much of an increase was necessary to bring these odors to the sensory threshold. Before the fast, B was unable to scent two and sometimes three of these odors when fully exposed, while after the fast he never scented asafoetida at all. In the case of D a smaller number of failures was found. This observation raises the old question whether children are more sensitive to odors than adults.

Fig. 1 illustrated the results of Table I. The dotted lines show the results during the fast. The sixteenth day for B is not quite so low as it should be in the figure.

The literature on experiments of this kind is meager. Most of it is the by-product of experiments similar to Marsh's and our own earlier study. Hoelzel and Kleitman, for example, discuss the increased sensitivity of the digestive tract in connection with increased gastric acidity during a fast.[5] They show that the digestive tract is more sensitive to pain during and immediately following a fast than at other times.

That this sensitivity should be reflected in the upper part of the digestive tract seems reasonable. The close relation of taste and smell is well known. In the case of leather and beeswax a burning sensation was noticed in the nasal tract. This did not apparently extend beyond the throat, in the region of the thorax. The pleasant odors of balsam apparently did not extend so deeply, but filled the nasal cavity with a fullness unlike that felt at other times.

In the absence of more experimental work of this kind it is difficult to formulate a theory as to why one should be more sensitive to odors during a fast. As between central and peripheral mechanisms we are inclined to attribute more importance to the latter. As between the rest of the alimentary tract and the olfactory bulbs the evidence, it seems to us, is also on the side of the latter, although the former may affect sensitivity in an indirect way. It has been suggested that the bipolar sensory cells located in the nasal mucous membrane are not necessarily more sensitive during a fast but the secretion covering the surface of the epithelium is more condensed. The gaseous particles entering into solution would make a more condensed solution and the corresponding increased stimulation may result.

It is well known that the greatest loss in weight during a period of fasting occurs in the first 3 or 4 days. The undigested food residue accounts for part of this, but not all. The liquid intake is greatly reduced during a fast, and the *S* has to force himself to drink a sufficient amount to meet the needs of the body. It is reasonable to suppose that the fluid about the mucous and serous cells that ordinarily covers the mucous membrane would be less with the decreased food and water supply. Hence the above assumption that the gaseous particles form a more condensed solution as they are absorbed by the liquid of the mucous membrane seems plausible.

Other Tests

In order to check the result of our earlier study we employed four other tests with B. The first two were tests of equilibrium and steadiness—the same as were used in the earlier work and administered in the same way; the third was a memory test; and the fourth a relations test.

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[5] F. Hoelzel and N. Kleitman, Some conditions affecting subjective and objective manifestations of hunger, *Arch. Inter. Med.*, 39, 1927, 710-740.

Equilibrium. The test of equilibrium, as before, gave no definite nor significant results (see Table II).

Steadiness. The steadiness test gave positive results after the first 5 days, *i.e.* a constant increase in steadiness as the fast progressed. These results are shown in Fig. 2. The averages in Table II show no gain during the fast because the marked unsteadiness the first few days of fasting tend to obliterate the actual increase. The holes in the metal plate, in this study, were somewhat smaller than in the earlier work, and that is why more errors are recorded in the present study. This change improved the test since the smaller the holes the more definite the results.

Memory. In order to measure more adequately the 'central processes' we prepared a test from our classification of nonsense syllables.[6] We took an equal number of syllables from the 8 divisions that are represented

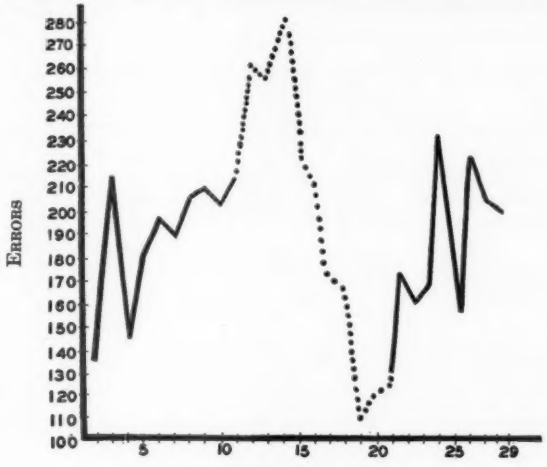

FIG. 2. STEADINESS TEST
Results of B. Broken lines show results of the fast

from 0–40% inclusive. These syllables are difficult to memorize and although meaning can be attached to some of them, most of them meant nothing for the *Ss*. Sixteen of these were used daily. They were presented one every 2 sec. for 10 readings at the beginnings of the group of tests. After an interval of 25 min., B was tested for the number retained. The interval was occupied with other tests arranged in such a manner that the olfactory test came before the test employing nonsense syllables. It is evident that we could stop the olfactory test at any place, to return to it later, without invalidating the results. In case of approaching adaptation an interval of rest was interpolated, but this never occurred in this test (olfactory) before the memory test. After a measure of the syllables retained we proceeded with successive presentations until all of the syllables were memorized. This gave us two sets of data, as can be seen in Table II.

[6] J. A. Glaze, The association value of nonsense syllables, *Ped. Sem.*, 35, 1928, 255-267.

The anticipation method was used. In view of the post-fast performance it can be said that nothing definitely in the way of increased mental acuity is shown during the fast.

Relation. A different process is represented by what we have chosen to call the 'relations test.' This test was made up of 5 consonants arranged in the form of a nonsense word, the following being examples: MWGKX, HGKZD, FNZKB, ZBJVN, VBGSX, ZSFMK, DGFBH. This type of test has been used before in the Michigan laboratory.[7] The problem consisted in (a) indicating every 'word' that contained three consecutive letters not found in the preceding 'word,' and (b) indicating every two adjacent 'words' that had two consecutive letters in common. Every nonsense word was placed on a card, and 120 cards were presented every day, requiring 238 judgments. A 5-sec. exposure time was used. The results of this test are shown in Table II.

TABLE II

PERFORMANCES OF B IN FOUR TESTS DURING THE PRE-FAST, FAST, AND POST-FAST PERIODS

mm = millimeters movement; c = contacts

Test		Pre-Fast		Fast		Post-Fast	
		Av.	m.v.	Av.	m.v.	Av.	m.v.
Equilibrium	Blindfold	68.8 mm	8.8	66.6 mm	13.2	63.6 mm	13.1
	Seeing	56.2 mm	9.3	58.9 mm	10.4	44.5 mm	12.3
	Av.	62.7 mm	8.9	62.8 mm	6.0	59.7 mm	10.2
Steadiness (hands)	Right	84.4 c	15.0	93.7 c	32.9	91.6 c	16.4
	Left	98.4 c	12.2	94.5 c	24.1	95.4 c	10.5
	Av.	94.4 c	10.2	95.6 c	26.7	93.5 c	11.8
Memory	Savings	3.25%	1.9	5.1%	1.5	5.9%	2.0
	100% learning	12.6	5.1	10.4	2.4	8.5	2.5
Relations correct		92.79%	2.79	94.79%	2.25	98.28%	0.9

SUPPLEMENTARY EXPERIMENTS

(1) *Sensitivity before and after meals.* Evidence was gained in a supplementary experiment that showed *S*s are more sensitive to odors before a meal than after it. Seven students in elementary psychology (6 sophomores and 1 senior) were tested with the 7 odors mentioned above. The *S*s agreed to eat normally, to forego eating or chewing anything, or drinking anything that contained odorous or food substances between 8:00 A.M. and noon, the testing hour. A prohibition was also placed on smoking during these hours. After the first tests the *S*s (only 2 a day could be tested) went to lunch and returned immediately afterward for the second tests. Each *S* was tested 6 days. The results of the first day, so as to eliminate the effects of practice, were discarded. The effects of practice, however, did not appear to be very marked. The results are shown in Table III.

All the *S*s, with the exceptions to be noted, ate a light breakfast and a fairly heavy lunch. G.R. went without breakfast, and I.B. and D.H. ate light lunches, usually nothing more than a sandwich and a cup of chocolate.

[7] C. P. Wang, *The General Value of Visual Sense Training in Children*, 1926, 68.

(2) *Retest with D.* Six days after the experiment with D ended we decided to test him by imposing another 5-day fast. He was available and apparently willing, but was very anxious for the fast to end after it was under way. Four days of preliminary testing was done and 5 days follow-up. No significant results were secured.[8]

TABLE III

SENSITIVITY TO ODORS BEFORE AND AFTER THE NOON MEAL
Sensitivity expressed in mm. of odoriferous material exposed

Time of test	*Ss*						
	R.E.	R.S.	J.H.	H.A.	I.B.	D.H.	G.R.
Before meal	46.1	61.8	63.7	27.4	36.5	28.2	28.9
After meal	93.0	68.3	66.9	34.9	39.0	26.3	28.3

CONCLUSION

We conclude from the results of this study:

(1) That there is a marked increase in the sensitivity to odors during a fast.

(2) That steadiness, measured by the pointing test, tends to increase as the fast progresses.

(3) That the higher mental processes are not improved while the fast is in progress.

(4) That the most pronounced success, both in mental and muscular activity, is attained during the post-fast period.

[8] It was a mistake to use this *S* again so soon after the first fast. The intimacy of contact in the previous fast had its disadvantages. On the fourth day of the fast, for example, he came to the laboratory (on the same floor as his ward) chewing gum. He had secured it from some boys in his ward. He became somewhat secretive about the matter when questioned after the test. The results for the retest are: pre-fast, 32.4 ± 5.4; fast, 35.3 ± 3.5; post-fast, 34.1 ± 3.8.

THE LIGHT REACTIONS OF THE MEAL WORM (*TENEBRIO MOLITOR* LINN)

By CHARLES EDWARD BALFOUR, Princeton University, and LEONARD CARMICHAEL, Brown University

The object of the experiments reported in this paper was to determine the reactions of the meal worm, *Tenebrio molitor* Linn, to certain light conditions.[1]

The meal worm is a yellow, waxy, slender larva of an average length of 2 cm. In adult form it is a 'darkling' beetle approximately 1½ cm. in length with wing covers which are rigid lengthwise. One generation a year is said to be normal. The beetle is nocturnal. The white, bean-shaped eggs are approximately 1 mm. in length. They are deposited in meal. The larvae mature in about three months and the pupa stage has a duration of approximately three weeks.[2]

No extensive work has been done on the light reactions of *Tenebrio molitor*. In 1893 Loeb characterized this type as negatively heliotropic,[3] and in 1905 he reported a rather casual study of the meal worm.[4] He notes that if the animals are placed upon a plate near a window they will creep to the dark side of the receptacle. If the plate be covered with dark blue glass the result of the experiment is the same, but under red glass the larvae behave much as they do in the dark. If the plate be covered one half with red glass and one half with blue glass the animals will move from that part of the plate under the blue glass to that under the red glass, but not from the portion of the receptacle under the red glass to that under the blue. If one half of the plate is covered with red glass and the other half with opaque cardboard the animals scatter in all directions, but at length the greater number collect under the cardboard. All of these experiments were done in direct sunlight. Loeb noticed that the meal worms show a marked stereotropism and tend to collect in the corners of boxes. He also noted that the animals avoid or "flee from moisture and seek dry spots."[5]

It is easy to substantiate the general fact of what Loeb has called the heliotropism of this organism. The present writers have frequently noticed at night, on entering the laboratory room in which the meal containing the worms was kept in a glass jar, that, immediately after turning on the light, a large number of worms may be observed upon the exposed surface of the meal. In the light the organisms at once began to burrow back into the meal and in a few seconds none were visible. The experiments reported below were undertaken in an effort to give a more exact description of certain responses of the meal worm.

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*Accepted for publication November 30, 1927.

[1]The authors are indebted to Professor G. H. Parker of Harvard University for suggesting this problem. The major portion of the experimental work was carried out in the psychological laboratory of Princeton University.

[2]The facts of the natural history of *Tenebrio molitor* Linn given above are based upon one of the few accounts of this organism available. Cf. W. Lockhead, *Class Book of Economic Entomology*, 1919. Most of the facts presented, however, have been verified by the authors who have studied the material through a period of several years in the laboratory.

[3]J. Loeb, Ueber künstliche Umwandlung positiv heliotropischer Thiere in negativ heliotropische und umgekehrt, *Arch. f. d. ges. Physiol.*, 54, 1893, 102.

[4]Loeb, *Studies in General Physiology*, 1905, 70 f.

[5]*Loc. cit.*

Experimental

Apparatus. In order to study the light reactions of the meal worm under controlled conditions a simple maze was constructed consisting of three alley-ways in the form of the letter 'T.' This maze was so arranged that the worm, on being placed in the central alley, could move forward for a distance of approximately 10 cm. before it came to the end of the alley. At the end of this entrance alley were openings into two alleys which lead to the right and to the left respectively and together formed the top of the 'T.' Each of these two upper alleys was 10 cm. in length. The walls of all alleys were approximately 5 cm. in height. The alleys themselves were ½ cm. in width. The maze was so arranged that electric lights could be flashed on at all three openings of the 'T.' Thus by appropriate switches the end of the right or left alley could be illuminated. A light was also placed behind the entrance alley for it was found that this device assisted in directing the animal to move into the maze. These lights consisted of small 6.2-volt Mazda bulbs. The entire maze was constructed of wood and was evenly painted a dull black.

Procedure. Before any differential experiments were attempted, each worm was taken from darkness and run through the maze 10 times. During these preliminary trials all lights were extinguished save the one at the entrance alley. This precaution was taken because it was discovered that certain of the worms tended under neutral conditions to turn consistently either to the right or to the left. Only worms were selected for experimental study which, in a series of 10 trials, turned at least 5 times toward the alley that was to be lighted in that particular experimental series. By observing this precaution it was rendered more probable that the differential action observed in the worm was due to the light, rather than to a former habit or some intrinsic structural or physiological peculiarity of the particular worm which determined it to turn predominately either to the right or to the left. It is a curious fact that something analogous to the human condition of right- and left-handedness was discovered to exist in these larvae.

Each larva thus selected was run through the 'T' apparatus 20 times with light on the right or on the left side, as the case might be. The larvae were inserted into the entrance alley and gently stimulated by the light as already noted. In some cases a small camel's hair brush was also used to stimulate the animal. As indicated above by Loeb, the larvae were decidedly stereotropic and this response in the larger worms tended to retard their progress through the alleys of the maze. In general the smaller worms moved more rapidly than did the large ones.

When a worm reached the point at which the entrance alley joined the two alleys forming the cross bar of the 'T,' one side of its anterior segments was illuminated by the light placed at the right or left exit as the case might be. The behavior of the animals in this situation was by no means uniform. The same worm did not always give the same reaction at this point nor did any two worms necessarily behave in the same manner. It was possible, however, roughly to classify the behavior of the worms at this point into six general types. These types are indicated in the table as *A*, *B*, *C*, *D*, *E*, and *F*. Behavior that eventually resulted in turning away from the light is characterized as either *A*, *B*, or *C*. That which resulted in turning the animal toward the light is termed *D*, *E*, or *F*. It must be understood that these six types of behavior are in no sense considered anything more than an essentially arbitrary effort on the part of the authors to give some quantitative estimate of the nature of the behavior of the larvae as seen in the specific situation described.

In explanation of the behavior of the classes designated by the letters appearing in Tables I and III, the following definitions may be listed:

A. This activity includes those responses in which the worms turned and went down the dark alley, directly, without moving toward the light at all.

B. This activity includes those responses in which the worms first moved a few segments into the lighted alley and made slight movements with the anterior segments, before definitely turning into the dark alley.

C. This activity includes those responses in which the worms moved for a few segments into the darkened or lighted passage a number of times before eventually moving into the darkened passage.

D. This activity includes those responses which are the reverse of the responses included in class *C*. In this activity after the worms had moved for a few body-segments into the darkened or lighted passage a number of times, they eventually moved into the lighted passage.

E. This activity includes those responses which are the reverse of the responses included in class *B*. In this activity after the worms had moved for a few body segments into the dark alley, they turned and went definitely into the lighted passageway.

F. This activity includes those responses which are the reverse of the responses included in class *A*. In this case the worms moved directly from the entrance alley into the lighted passage.

Results. The results of these maze experiments are summarized in Tables I–IV.

Table I uses the letters as indicated above. In this table the reactions of the 21 worms of the first experimental series are shown in detail. In the first vertical column (reading from left to right) is given the number assigned to each worm. In the second column, headed "No Light," is given the reactions of each worm to the maze when both the right and the left alleys were unilluminated. The next columns show the reactions made by each worm to 20 trials in each one of which the light was on the right side.

Table II summarizes the results given in full in Table I. The numbers in the vertical columns indicate the frequency in the twenty trials of each of the types of behavior listed above as *A*, *B*, *C*, *D*, *E*, and *F*. The group of columns labeled "first half" and "second half" respectively show the difference between the first 10 trials and the second 10 trials. Below the individual columns is given the sum of the frequency of the 6 classes of behavior. These sums are also shown as percentages of the total number of trials. Finally in the bottom line is given the percentage of trials which led away from the light and those which led into it. It will be seen that in approximately 4 out of every 5 trials the worms turned away from the light. Little significant difference is to be noted between the first and the second series of 10 trials. In other words, the present experiments give no real indication of a practice effect, although they do not demonstrate that such an effect is absent.

Table III shows the result of a series of check experiments in which the light was on the left and not on the right side. In other respects this table may be read like Table I, above.

TABLE I

DETAILED RESULTS OF EXPERIMENTAL SERIES, WITH LIGHT ON RIGHT SIDE OF MAZE

Worm	No Light		Trials: Light on Right																			
	R	L	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	5	5	*F*	*A*	*C*	*C*	*E*	*A*	*A*	*B*	*A*	*A*	*B*	*F*	*A*	*C*	*B*	*A*	*A*	*B*	*E*	*A*
2	7	3	*B*	*B*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*B*	*B*	*C*	*B*	*B*	*B*	*B*	*B*	*B*	*C*	*C*
3	5	5	*A*	*A*	*A*	*D*	*B*	*B*	*A*	*A*	*B*	*A*	*E*	*B*	*B*	*A*	*B*	*B*	*A*	*B*	*B*	*A*
4	8	2	*B*	*B*	*D*	*A*	*B*	*B*	*A*	*B*	*B*	*B*	*B*	*B*	*B*	*B*	*D*	*D*	*A*	*B*	*B*	*A*
5	8	2	*F*	*F*	*D*	*C*	*B*	*A*	*B*	*B*	*B*	*B*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*B*	*F*	*C*
6	10	0	*F*	*B*	*B*	*F*	*B*	*F*	*B*	*B*	*F*	*F*	*B*	*B*	*B*	*D*	*B*	*F*	*B*	*F*	*F*	*D*
7	5	5	*B*	*D*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*B*	*E*	*A*	*A*	*A*	*A*	*A*	*A*	*B*
8	10	0	*F*	*F*	*F*	*B*	*F*	*F*	*F*	*B*	*F*	*F*	*F*	*F*	*F*	*B*	*B*	*A*	*B*	*B*	*F*	*B*
9	6	4	*A*	*F*	*A*	*F*	*B*	*F*	*B*	*A*	*B*	*B*	*F*	*A*	*B*	*B*	*B*	*B*	*B*	*B*	*B*	*B*
10	5	5	*B*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*C*	*B*	*A*	*C*	*A*	*B*	*A*	*A*	*C*	*A*	*B*
11	5	5	*B*	*E*	*B*	*A*	*F*	*A*	*A*	*A*	*A*	*F*	*F*	*F*	*A*	*A*	*B*	*A*	*A*	*A*	*A*	*A*
12	9	1	*B*	*F*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*B*	*B*	*F*	*B*	*F*	*B*	*B*	*F*	*F*	*B*	*B*
13	9	1	*B*	*B*	*A*	*A*	*F*	*A*	*B*	*B*	*F*	*B*	*A*	*B*	*B*	*F*	*A*	*A*	*B*	*B*	*F*	*B*
14	8	2	*B*	*C*	*C*	*A*	*C*	*C*	*C*	*C*	*D*	*A*	*C*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*C*	*A*
15	5	5	*B*	*B*	*A*	*F*	*B*	*C*	*A*	*B*	*B*	*A*	*A*	*A*	*A*	*B*	*A*	*A*	*B*	*C*	*B*	*B*
16	8	2	*B*	*F*	*B*	*F*	*D*	*F*	*A*	*A*	*C*	*B*	*B*	*F*	*B*	*C*	*B*	*B*	*A*	*C*	*B*	*B*
17	7	3	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*F*	*F*	*A*	*A*	*B*	*A*	*A*	*B*	*C*	*F*	*A*
18	7	3	*A*	*B*	*B*	*A*	*B*	*B*	*F*	*B*	*A*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*F*	*A*	*A*	*B*
19	7	3	*A*	*A*	*F*	*B*	*B*	*A*	*A*	*B*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*F*	*B*	*F*	*A*	*A*
20	8	2	*B*	*A*	*A*	*A*	*A*	*A*	*B*	*F*	*B*	*F*	*B*	*B*	*A*	*A*	*A*	*A*	*B*	*A*	*A*	*A*
21	10	0	*A*	*A*	*A*	*F*	*A*	*A*	*A*	*B*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*A*	*F*	*A*	*A*	*A*

TABLE II

SUMMARY AND ANALYSIS OF RESULTS OF EXPERIMENTAL SERIES SHOWN IN TABLE I

Worm	End Reations (20)						First Half (10)						Second Half (10)					
	A	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*
1	9	4	3	0	2	2	5	1	2	0	1	1	4	3	1	0	1	1
2	0	16	3	0	0	1	0	10	0	0	0	0	0	6	3	0	0	1
3	9	9	0	1	1	0	6	3	0	1	0	0	3	6	0	0	1	0
4	4	13	0	3	0	0	2	7	0	1	0	0	2	6	0	2	0	0
5	1	12	2	1	0	4	1	5	1	1	0	2	0	7	1	0	0	2
6	0	10	0	2	0	8	0	5	0	0	0	5	0	5	0	2	0	3
7	15	3	0	1	1	0	8	1	0	1	0	0	7	2	0	0	1	0
8	1	7	0	0	0	12	1	2	0	0	0	7	1	5	0	0	0	4
9	4	11	0	0	0	5	3	4	0	0	0	3	1	7	0	0	0	2
10	13	3	3	1	0	0	8	1	1	0	0	0	5	2	2	1	0	0
11	12	3	0	0	1	4	5	2	0	0	1	2	7	1	0	0	0	2
12	0	14	0	0	0	6	0	8	0	0	0	2	0	6	0	0	0	4
13	6	10	0	0	0	4	3	5	0	0	0	2	3	5	0	0	0	2
14	10	1	8	1	0	0	2	1	6	1	0	0	8	0	2	0	0	0
15	8	9	2	0	0	1	3	5	1	0	0	1	5	4	1	0	0	0
16	3	9	3	1	0	4	2	3	1	1	0	3	1	6	2	0	0	1
17	14	2	1	0	0	3	9	0	0	0	0	1	5	2	1	0	0	2
18	5	12	0	0	0	3	3	6	0	0	0	1	2	6	0	0	0	2
19	13	4	0	0	0	3	6	3	0	0	0	1	7	1	0	0	0	2
20	12	6	0	0	0	2	5	3	0	0	0	2	7	3	0	0	0	0
21	17	1	0	0	0	2	8	1	0	0	0	1	9	0	0	0	0	1
Individual Sums	156	159	25	11	5	64	80	76	12	6	2	34	77	83	13	5	3	29
Trials Total						420						210						210
% of Each Reaction	37	37	06	02	01	15												
Totals		L 340			80 R			L 168			42 R			L 173			37 R	
% of Gross Bodily Reactions		80.95			19.05			80			20			82.38			17.62	

TABLE III

DETAILED RESULTS OF CHECK SERIES WITH LIGHT ON LEFT SIDE OF MAZE

Worm	No Light		Trials: Light on Left																			
	L	R	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
C–1	9	1	*B*	*B*	*B*	*A*	*B*	*B*	*A*	*A*	*A*	*A*	*F*	*B*	*A*	*B*	*B*	*B*	*B*	*B*	*A*	*B*
C–2	6	4	*A*	*A*	*A*	*B*	*B*	*B*	*F*	*B*	*B*	*B*	*B*	*A*	*B*	*A*	*A*	*A*	*A*	*A*	*A*	*B*
C–3	8	2	*A*	*F*	*A*	*A*	*B*	*A*	*F*	*F*	*A*	*B*	*B*	*B*	*B*	*B*	*F*	*F*	*A*	*F*	*A*	*B*
C–4	9	1	*F*	*A*	*A*	*A*	*F*	*B*	*X*	*B*	*A*	*C*	*B*	*A*	*B*	*F*	*B*	*C*	*A*	*B*	*F*	*B*
C–5	6	1	*F*	*C*	*C*	*C*	*C*	*F*	*E*	*A*	*B*	*C*	*E*	*B*	*B*	*A*	*A*	*A*	*A*	*F*	*F*	*C*

TABLE IV

SUMMARY AND ANALYSIS OF RESULTS OF CHECK SERIES SHOWN IN TABLE III

Worm	End Reactions (20)						First Half (10)						Second Half (10)					
	A	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*	*A*	*B*	*C*	*D*	*E*	*F*
C–1	7	12	0	0	0	1	5	5	0	0	0	0	2	7	0	0	0	1
C–2	10	9	0	0	0	1	3	6	0	0	0	1	7	3	0	0	0	0
C–3	7	7	0	0	0	6	5	2	0	0	0	3	2	5	0	0	0	3
C–4	6	7	2	0	0	4	4	2	1	0	0	2	2	5	1	0	0	2
C–5	5	3	6	0	2	4	1	1	5	0	1	2	4	2	1	0	1	2
Individuals Sums	35	38	8	0	2	16	18	16	6	0	1	8	17	22	2	0	1	8
Trials Total						99						49						50
% of Each Reaction	35	38	08	0	02	16												
Totals	R 81			18 L			R 40			9 L			R 41			9 L		
% of Gross Bodily Reactions	81.82			18.18			81.64			18.37			82			18		

Table IV shows the summary of the results given completely in Table III. It should be read like Table II. The fact should be noted that under the conditions of this check series the worms turned away from the light in approximately 4 out of every 5 trials.

Beside these experiments in the 'T' maze other observations were made upon the light reactions of the meal worm.

The tube and objective of a small compound microscope was fastened on the end of a pocket flash-light. When the light was "turned on," this device produced a fine pencil of light between 1–2 mm. in diam. When a meal worm was placed upon a table in a dark room it was found possible to direct its movements in a most automatic fashion by turning this pencil of light upon the right or left side of the anterior terminal segment of the worm. The reactions of the worm under these conditions were so direct and stereotyped that they indeed suggest Hammond's "Electric Dog," an artificial heliotropic machine described by Loeb.[6] The anterior segment of the worm was the only segment which seemed subject to this stimulation. It seems indeed that the light receptors of this organism are concentrated in this region. Small pigment spots are probably the locus of the receptive function. Our observations show that there is little reason to suppose that the entire integument of the worm is in any marked degree sensitive to light as is the case in certain other organisms.[7]

The localization of the light receptor in the anterior segment was indicated in still another manner. Ten worms were selected which showed no marked tendency to turn normally to the right or to the left when allowed to move away from the strong daylight. One side of the head segment of each of these worms was then painted with a black opaque varnish. In 5 worms the right side was painted and in 5 the left side was so treated. In approximately 4 out of 5 cases in over 100 trials painted worms turned so as to bring the black varnish pigment spot toward the source of light. That is, if the left spot was varnished the worm would turn to the left, and if the right side was varnished the worm would turn to the right. The varnish used was made of collodion and lampblack. A number of worms were painted with a transparent coating of collodion on one side of the head, but this had no effect that could be observed on their behavior. It is reasonable to suppose, therefore, that the absence of light produced by the black varnish was the cause of the behavior noted above. Worms with both sides of the head painted black when stimulated went with apparent indifference to the right or to the left.

The paths inscribed on smoked paper by a number of larvae were also studied. This method gave an accurate tracing of the path of the worm if the paper was not too densely smoked, and seems to be a better method for the type of behavior under consideration than would have been Mast's procedure of drawing with a fine pen in white ink upon the black paper the course of blowfly larvae, a result which he accomplished by keeping the pen about a centimeter in front of the organism.[8]

When a worm was moving in a direct path and a light was suddenly flashed on to the right or left of the animal, it was found that in practically all cases a curve would be described in which the animal oriented itself away from the new source of light. In these cases the light of the bulb at the rear was extinguished when the new source of illumination was turned on.

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[6]Loeb, *Forced Movements, Tropisms, and Animal Conduct*, 1918, 68 f.
[7]Cf. A. Forel. *Das Sinnesleben der Insekten*, 1910, 45-79.
[8]S. O. Mast. *Light and the Behavior of Organisms*, 1911, 176 ff.

At present it is not possible to say with absolute assurance that all of the responses noted were to light and not to heat. It seems to the present investigators, however, improbable that heat reactions were involved to any great extent in the experiments recorded. This tentative conclusion is based upon the following observations: (1) There seems to be little reason why there should be a noticeable difference in the heat reactions of the larvae which had one side of the anterior segment painted with black varnish and those painted with translucent varnish. (2) The source of light used in these experiments was not such as to produce much heat. (3) We were able to show, moreover, that the worms will move away from light and into warm areas. A copper pipe full of live steam was arranged at right angles in front of a piece of glass tubing. The worms were placed in the opposite end of the tube and due to properly arranged light stimulation they moved forward in the tube. They continued this advance within a few mm. of the hot pipe before stopping. In some cases they even touched the hot pipe itself. This experiment seems to suggest, as have the other observations noted above, that in the present settings the worms are much more sensitive to light than to heat.

The worms that moved into the lighted alley in every case stopped before touching the lighted bulb and in all cases after a short delay began to back out of the lighted alley.

THEORETICAL

May *Tenebro molitor* be characterized on the basis of the observations noted above as a negatively heliotropic organism? The answer to this question obviously depends upon the definition of the word tropism that is adopted. Mast has listed fifteen tropism "theories," or, as they might better be called, definitions of the word 'tropism.'[9] In the table printed above the fluctuations of the response of the organisms are indicated and yet it is shown that the responses of the meal worm are in the main such that they will cause the animals to move away from the light. As Parker has said: "Anyone who watched carefully the orientation of an earthworm or a fly larva in a field of light will have noticed the many fluctuating movements of the head made by these animals during this process. As to the presence of these so-called trial and error movements there can be not the least question, but as to their significance there is, in the mind of the reviewer at least, great doubt; for in any animal that orients more or less directly these movements are apparently always subordinate to a principle essentially tropic."[10]

According to this view, then, there can be no doubt of the fact that the meal worm is negatively phototropic. The striking fact remains, however, that in approximately one-fifth of the cases, when all of the trials of many worms are averaged together, the response is not away from the light. Two of the

[9]Mast, *op. cit.* 53-57. This author also notes in a later publication that two other theories have been added to the list; *idem*, Discussion of certain questions raised by Parker in a review of "Light and the behavior of Organisms," *J. Animal Behavior*, 2, 1912, 209f.

[10]G. H. Parker, Mast's "Light and the behavior of organisms," *J. Animal Behavior*, 1, 1911, 462.

many possible explanations of this fact may be suggested here. First, in the main experimental series in the 'T' maze certain worms were used which had in the preliminary series shown a marked tendency to turn to the right. In such cases, if the right alley was illuminated some of these strikingly 'right-handed' organisms tended to follow their peculiar proclivity even though such turning lead them into the light. Secondly, the light of the maze may not have been strong enough to stimulate certain worms when some peculiar morphological change had taken place in their constitution, as, for example, abnormal thickening and hardening of the chitin of the head segment. In no case, however, as noted above, was a worm observed to touch the lighted bulb. Moreover, the activity of the larvae in the experiments noted above in which a pencil of light was turned upon one 'eye spot' was quite uniform and they might almost be characterized as negatively heliotropic in the strict sense of that term as used by Loeb, for they seemed in almost all cases compelled to turn and move away from the source of light.[11]

[11]Loeb, *Forced Movements, Tropisms, and Animal Conduct*, 1918, 48.

EFFECTS OF GENERAL DISTRACTION ON THE HIGHER THOUGHT PROCESSES[1]

By H. Birnet Hovey, University of Oregon

"I have long held the opinion," wrote Schopenhauer, "that the amount of noise which anyone can bear undisturbed stands in inverse proportion to his mental capacity, and may therefore be regarded as a pretty fair measure of it. . . . Noise is a torture to all intellectual people."[2] Although there has been considerable experimentation with the effects of distraction upon simple processes, nothing, to my knowledge, has been done to determine the effects of distraction upon the higher thought processes except by Tinker, who gave an intelligence test to 56 students under standard conditions and also during the intermittent ringing of two bells.[3] He found that the distractor neither aided nor hindered average performance, but that the better students seemed to be more hindered than the poorer ones. He also found indications of less dispersion under the distraction. As intelligence tests are generally thought to be measures of the capacity to solve problems, to handle abstractions, they may, then, be safely considered as measures of higher thought processes. To determine distraction effects it would therefore only be necessary to administer a reliable intelligence test under distraction conditions. Using this procedure the following problems have been investigated:[4] (1) effects of distraction on certain higher mental processes; (2) the relationship between susceptibility to distraction and intelligence as measured by the Army Alpha; (3) individual differences in ability to overcome distraction; and (4) how much, if at all, distraction affects the reliability of a mental test.

Cassel and Dallenbach found that distraction inhibited, lengthened, or facilitated the simple sensory reaction; the effect being dependent upon the conscious attitude of the observer during the distraction.[5] The passive attitude was conducive to constant sensory reaction of normal length; the active attitude to a slow and variable one. Morgan found in a study of the overcoming of distraction while pressing keys, that the initial effect of distraction was a retardation of speed which was later followed by accelera-

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[1]I wish to express my thanks to Professors Edmund S. Conklin and Howard R. Taylor for their valuable suggestions and guidance in the course of this investigation together with their reading of this MS.

[2]Quoted from Will Durant, *The Story of Philosophy*, 1926, 330.

[3]M. A. Tinker, Intelligence in an intelligence test with an auditory distractor, this Journal, 36, 1925, 467-468.

[4]In connection with a study of the relation between distraction effects and extro-introvert tendencies, we found our data to be applicable to such an investigation as this.

[5]E. E. Cassel and K. M. Dallenbach, The effect of auditory distraction upon the sensory reaction, this Journal, 29, 1918, 129-143.

tion.[6] Upon removal of the distractions the speed fell below normal. Greater nervous tension was evident. Morgan believed that the overcoming of distraction is due to lip movements which incidentally change the rate of breathing; the *Ss* who made the best scores used more vocal movements and showed a higher breathing ratio. Noise was found to have a dynamogenic effect, which may be expressed as a general tendency to meet resistance with an extra output of energy. Morgan thought that this might possibly affect the higher mental processes. Other workers have found distraction to be a spur to attention. Tinker found that distraction, which consisted of a bell ringing each time a stylus touched the sides of the apparatus used for testing motor control, did not interfere with performance; but rather that it served as a spur to attention.[7] Tanner and Anderson found that the sounds of a tuning fork intensified the sensations of *Ss* while trying to determine changes in colors which were presented.[8] Individual differences in ability to compensate for distraction seem not to have been experimentally determined. Dockeray, in an experiment in which *Ss* discriminated sounds under auditory and electric shock distraction, found indications that differences in this ability could be determined, but does not presume to have measured them.[9] Tinker's finding of less ability to compensate by the more intelligent *Ss* is based on too few cases to be conclusive.[10]

Method and Procedure

As a means of attacking the problems presented above, two groups of *Ss* were used; one as an experimental group, and the other as a control. Each group was a section of the sophomore class in General Psychology. The experimental group numbered 171 *Ss* and the control 123. Six tests of Army Examination Alpha, Form 8, were given under normal conditions to both groups. The same 6 tests of Form 7 were given under distraction conditions to the experimental group, and under normal conditions to the control group. The two forms were given one and one-half months apart in each instance.

The 6 tests from Army Alpha were presented to the *Ss* in the following order: test 4—opposites; test 2—arithmetic; test 5—disarranged sentences; test 6—number completion; test 7—analogies; and test 8—general information. These 6 tests were selected and used in this particular order for reasons connected with another phase of the study to be reported later. The procedure in giving the Alpha tests was similar to that used in the Army,[11] except for such alterations in the preliminary statements as were necessary to fit the particular groups tested and the experimental conditions. In the instructions for Form 7 given under distraction the *Ss* were told that there would be distractions and that they were to do their best regardless. None of the *Ss* knew what the nature of the experiment was to be until the instructions were read. The timing and scoring were the same as used in the Army.

The experimental group met for the distraction experiment at its regular class hour, but in a special room containing 200 individual desks arranged in nine rows. One professor and 8 advanced students in psychology assisted. The first took charge of the testing. Four of the students assisted

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[6] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 5, 1916, no. 35, 1-84.

[7] M. A. Tinker, A study of the relation of distracted motor performance to performance in an intelligence test, this Journal, 33, 1922, 578-583.

[8] A. Tanner and K. Anderson, Simultaneous sense stimulations, *Psychol. Rev.*, 3, 1896, 378-383.

[9] F. C. Dockeray, Attention, distraction and fatigue, *J. Comp. Psychol.*, 2, 1922, 331-370.

[10] Tinker, *op. cit.*, this Journal, 36, 1925, 467 f.

[11] C. S. Yoakum and R. M. Yerkes, *Army Mental Tests*, 1920, 63-69.

in the operation of the mechanical distractors. The other 4 students served as distractors themselves by performing specified stunts. The distractors used were: 7 bells and 5 buzzers;[12] 5500-watt spotlight;[13] 90,000-volt rotary spark gap;[14] phonograph; two adjustable organ pipes and three metal whistles, 14, 24, and 26 in. long;[15] 55-pound circular saw (36 in. in diam.) mounted on a wooden frame; mounted camera operated by a well-known photographer; and the four students performing stunts.

Several distractors were often used simultaneously. Sometimes but one or two were used, and at other times there were intervals of complete silence. More than one form of distraction was used during each of the six tests, and each form was used more than once throughout the experiment with the exception of the photographing. The production of a climactic effect was sought for; more than half of the various forms of distraction were used sometime during the last test. The intervals of quietude distributed throughout the experiment were usually followed by novel forms of stimuli to heighten the effect of the silence periods that were to follow. One assistant operated a keyboard (in a laboratory room hidden from the *Ss*) connected with the bells and buzzers. A small bell would ring in one part of the room followed by perhaps a buzzer in another part, then a very sonorous bell in still another place, and so on. The spotlight was flashed about during most of the tests. One assistant produced discordant noises with the rotary spark gap by pressing on a key. *Mary Lou* was played on the phonograph during part of the test and *Petrushka* during part of another. The organ pipes and whistles were operated by two assistants. Sometimes major chords were played and at other times discords. The circular saw was first beaten with a wooden club and later battered with iron bars—rhythmically for intervals and then unrhythmically. The camera was carried into the room and adjusted while the *Ss* worked on the last tests. Some instructions were then given the cameraman, to make sure that the *Ss* would be aware of the fact that a photograph was to be taken. With the exception of the bells and buzzers, all the distracting apparatuses were carried into the large room while the *Ss* were working. The phonograph, the pipes and whistles, and the saw, were first used in the adjoining room out of sight of the *Ss*. Each of these were also sounded while being carried into the large room.

During the second test of Alpha the 4 stunt men marched about, keeping step, on the floor above. Some of the *Ss* said afterwards that they interpreted the noises as being produced by a number greater than 4. This illusion was due to the passing of the sounds through a partially soundproof floor. Later the stunt men walked into the room unusually attired, and marched up and down the aisles. During the last test they walked into the room carrying formidable looking instruments which were unfamiliar to the *Ss*. The assistants pretended to make use of these. After every aisle was walked once the men returned to the small room and came

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[12]On the moulding of the special room were placed 5 buzzers and 7 bells approximately equally spaced. The bells were of the following dimensions: one was 4 inches in diameter, one was 12, one was 6, two were 8, and two were 2½. The buzzers were all the same size but varied in pitch. These bells and buzzers were so distributed that there was not a concentration of similar noises in any part of the room; 12 volts d.c. were used to operate them. A common ground wire led to each. From the other terminal of every instrument a separate wire led to a keyboard in an adjoining laboratory room.

[13]The spotlight was used in the rear of the room so that the beams could be thrown on the walls, on the backs of the *Ss* and on the furniture, thus preventing them from shining in the *Ss*' eyes. The focus of the light could be reduced to a small diameter or spread over a large area. One man attended to this machine throughout the test but operated it only for intervals, according to instructions given him.

[14]The rotary spark gap is an apparatus which makes an unusual crackling noise which was easily heard throughout the room.

[15]The organ pipes and whistles were attached to a source of compressed air that was constantly regenerated. The pitch of these could be varied by moving their pistons.

back, each carrying a nail keg. They rolled the kegs down to the ends of the aisles, picked them up, and carried them to the front. This was continued until all the aisles were used several times.

In order to make the experimental and control groups comparable, individuals of one group were paired off with individuals of the other. The pairing was done on the basis of raw score totals individually obtained on Alpha, Form 8. For each score obtained in the control group, a score of similar size obtained by an individual in the experimental group, was sought for. Whenever a score in this second group could be found that differed two points or less from the score under consideration in the first group, these two scores were isolated as a pair. Each of the 123 individual scores in the control group were thus considered. Ninety out of this number could be successfully matched with 90 of the 171 scores in the experimental group. Thus 90 pairs were isolated, there being in no case a difference of more than two points between members of a pair. The correlation between the two paired sets indicates the closeness of the pairing: $r = 0.999 \pm 0.0$.

Results and Discussion

(1) The data obtained indicate some results with reference to efficiency in thinking. It was believed that if individuals are disturbed by distraction they would make scores lower than they would otherwise. The mean score of the experimental group on Alpha, Form 8, given under quiet conditions was 127.5 ±1.3; under distraction this was raised to 133.9 ±1.2 on Form 7. The control group made a mean score of 127.5 ±1.3 on Form 8, and one of 137.6 ±1.2 on Form 7. The improvement under distraction was 6.4 as compared to 10.1 under normal conditions. Thus the control group did better than the experimental by 3.7 ±1.7. The difference is probably significant but not large enough to warrant the inference that distraction greatly impedes thinking. It shows, conversely, that distraction is not a serious handicap to the group as a whole during 19 min. of concentrated thinking (19 min. were spent in actual work on the test problems). A longer and more exacting examination might, however, show much greater effects. Undoubtedly the onset of fatigue from the added output of energy, which Morgan found to be the situation in the overcoming of this distraction, would appear later if the testing were prolonged. But then one might be measuring the effects of fatigue on ideational processes, rather than the direct effects of distraction; and also perhaps indirectly measuring fatigue produced by distraction. Although the *Ss* in our experiment were under considerable stress during the 19 min., there may have been compensating factors. The results on the arithmetic test revealed that it is possible for even problem solving to be performed under a bombardment of irrelevant stimuli without much loss in accuracy and speed. The mean scores for the experimental and control groups on this test were, respectively, 11.4 and 11.3 on Form 8; and 12.2 and 12.7 on Form 7.

Comparison of total score averages of the two forms of Alpha indicate that distraction does not raise the level of total output for the group taken as a whole. Compensation evidently does not reach a stage of overcompensation. If an individual works to his full capacity on a mental test perhaps nothing can make him work better, though if he is not maintaining his maximum the distraction may serve as an incentive. This is the only way the writer can account for one finding. The correlation between the two forms of Alpha is higher within the experimental group than it is in the control. The two are, respectively, $r=0.87$ and $r=0.84$. It is not altogether unreasonable to suppose that the reliability of one measure as based on another is higher if incentive is maintained at an optimum in either or both of the tests than it would be otherwise. If two similar forms of a test can be considered perfect measures of the same traits, the correlation should be perfect, and the nearer the correlation between such measures approaches 1, the more nearly may the tests be considered true measures of the traits. To the extent that the difference between 0.87 and 0.84 is significant, we are justified in concluding that distraction spurs students to a performance which is nearer to their true ability. Now with two groups so exactly paired that the r between the paired individuals on the same form of Alpha is 0.999, we found that the difference between the rs in the two groups has some significance. The significance of this difference may be determined by the standard deviation of such differences; which was found to be 0.02.[16] The obtained difference, which is 0.03, divided by the standard deviation is 1.5. On the basis of 0.02 as sigma on a distribution curve of these differences, 1.5 would include about 86% of all possible differences, leaving 14% of possibilities beyond this point. Hence there are 86 chances to 14, or a little better than six chances to one, that the true difference is greater than zero, or that students more nearly approximate their maximum ability under distraction.

(2) We found that the more intelligent are as capable of thinking under distraction as the less intelligent, contrary to Schopenhauer's thesis. In fact, there are slight indications of the converse. This was shown when Alpha initial scores (intelligence scores) were compared with gain, or increase in score in the second test over the first, under distraction and under normal conditions. The relationship between intelligence scores and gain in Form 7 under distraction, over Form 8 was, $r= -0.31 \pm 0.06$ (experimental) and only $r= -0.38 \pm 0.06$

[16] For the method of computation see T. L. Kelley, *Statistical Methods*, 1923, formulae 128 and 139.

(control) under normal conditions. It should be said at this point that the negative coefficients are not infallible indications of a negative relationship between gain and initial ability. Thorndike explains that in general, imperfection in the original measures will distort the relationship between true initial ability and true gain making it more negative.[17] Past estimates of this relationship, he says, are all underestimates. We believe that the negative coefficients we obtained were due to the greater difficulty of originally high scores being exceeded. We are concerned here, however, only with relative gain in the two groups.

(3) Since the correlation between Forms 8 and 7 in the experimental group is not lower than it is in the control, the situation must be that shifts in rank under distraction are no greater than they are under normal conditions. This indicates that individual differences in ability to resist distraction are not very great. The spread in the group tested under distraction is no greater than it is in the other group. The standard deviation of the control group on Form 7 is $\sigma = 17.23 \pm 0.87$, and $\sigma = 16.71 \pm 0.84$ for the experimental. The comparative range of improvement in Form 7 for the experimental group is also smaller than it is for the control. The standard deviations of the differences between Forms 8 and 7 (gains and losses) for the experimental is $\sigma = 8.93 \pm 0.45$, which can be compared with that of the control, $\sigma = 10.48 \pm 0.53$. So the variability in the amounts of improvement was actually less under distraction than it was otherwise. Thus 19 min. of testing the higher thought processes under distraction yields no evidence at all of individual differences in susceptibility to distraction. Even with sufficient nervous strain to cause crying spells afterwards, in the cases of two girls, there was average improvement in one test score, and better than average in the other.

(4) Our results bring out another fact which is at least interesting. Quiet and freedom from disturbance are supposed to be necessary features of good test procedure because voluntary attention is an important factor in all so-called intelligence tests. If there are no disturbing effects of distraction there is little need of guarding against it in mental testing procedure. If there are effects, they must, of course, be considered in the testing technique. Attention has been drawn to the fact that distraction actually raised the reliability of Alpha. Some significance, as has been mentioned, can also be attached to the difference between the sigmas of gains and losses in the two paired groups. The difference between these sigmas is 1.55.

[17] E. L. Thorndike, The influence of the chance imperfections of measures upon the relation of initial score to gain or loss, *J. Exper. Psychol.*, 7, 1924, 225-232.

The sigma of such differences is 1.03, making it about 86 possibilities in 100, or better than six chances to one, of the true difference being greater than zero. When true ability within a group is measured, the spread is smaller than it would be otherwise. Therefore it can be said that perhaps testing under distraction in our experiment approached the measurement of true ability to a greater extent than testing under standard conditions. It is more consistent to explain this as being due to the induction of an increased incentive by the experimental situation rather than as a direct effect of the distraction.

SUMMARY

Some effects of general distraction on higher mental processes have been experimentally investigated. This was done by giving a mental test to a group of college students under standard conditions and a re-test under severe general distraction. A control group was given the same tests without the use of distraction. The following inferences seem justified from the data: (1) Higher mental processes are comparatively unimpeded by distraction. (2) Intelligence is not related to susceptibility to distraction. (3) There are no individual differences in susceptibilty to distraction. (4) True mental ability is more nearly approximated under distraction than under standard conditions.

THE RELATION BETWEEN DIFFERENT MEASURES OF ABILITY TO REPORT

By JOHN A. McGEOCH, Washington University

It is difficult to evaluate work in the psychology of testimony without knowing the extent of the correlation between different methods of reporting upon the same material, and between similar methods of reporting upon different materials. In order to work out these relationships, Pearson product-moment coefficients of correlation have been computed, in this paper,[1] between the narrative and interrogatory forms of report on each of three different report materials, and between narrative and narrative, interrogatory and interrogatory, on the three materials. These materials were the Binet object-card, a picture, and an event.[2]

The most significant scores for this purpose are total items right and wrong. The correlations between these scores on narrative and interrogatory are given in Table I. Each age group has been treated separately.

TABLE I

CORRELATIONS BETWEEN NARRATIVE AND INTERROGATORY

Age	Total Items Right			Total Items Wrong		
	Object-card	Picture	Event	Object-card	Picture	Event
9	0.397	0.000	0.397	0.304	0.073	0.126
10	0.157	0.196	0.419	0.341	0.127	0.118
11	0.346	0.194	0.278	−0.077	−0.074	−0.041
12	0.404	0.385	0.185	0.205	0.361	−0.021
13	0.352	0.258	0.633	9.221	0.273	0.177
14	0.454	0.352	0.658	0.224	0.042	0.211
Mean	0.351	0.230	0.428	0.203	0.133	0.095

The coefficients of correlation between items right on narrative and interrogatory are all positive, with the exception of one which is zero; they range from 0–0.658. Their probable errors have been computed, and all of the coefficients save a few of the smallest are four times their probable errors. The mean for each type of report material shows in a single figure the general tendency of the results. In order of increasing size, the

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[1]From the Psychological Laboratory of the University of Chicago.

[2]A description of these report materials and of the experimental procedures used will be found in J. A. McGeoch, The fidelity of report of normal and subnormal children, this JOURNAL, 36, 1925, 434-445. The numbers of *Ss* and their intellectual status have been given in a preceding paper on age and sex differences, this JOURNAL, 40, 1928, 458-466.

means run: picture, object-card, event. The means represent the general tendency with some accuracy and indicate that there is a low but definite tendency for the scores to be related positively.

There are evident variations with age in the size of the coefficients. These may be due to too many chance factors to warrant attention here, since the main problem is not age differences in correlation but the general tendency of relation between different report methods. Suffice it to note a slight tendency for the size of the coefficients to increase with age.

The coefficients of correlation between items wrong are much lower than those between items right. They are, with a few exceptions, positive. The means run in order of increasing size, event, picture, object-card, but they are so small as to indicate a negligible relationship.

TABLE II

CORRELATIONS BETWEEN REPORT SCORES: ITEMS RIGHT

Age	Narrative			Interrogatory		
	Object-card Picture	Object-card Event	Picture Event	Object-card Picture	Object-card Event	Picture Event
9	0.470	0.000	0.211	0.232	0.303	0.511
10	0.442	0.293	0.072	0.296	0.204	0.369
11	0.372	0.276	0.237	0.217	0.051	0.217
12	0.395	0.268	0.664	0.280	0.246	0.122
13	0.284	−0.485	0.853	0.116	0.092	0.161
14	0.467	0.104	0.554	0.489	0.371	0.564
Mean	0.405	0.076	0.431	0.271	0.211	0.324

Partial correlations have been computed, with intelligence held constant, with the result that the size of the coefficients is reduced, but only slightly.

Coefficients of correlation between similar methods of reporting upon different report materials are given in Table II.

The coefficients of correlation between the object-card and picture narratives, at every age, indicate a positive and in most cases a moderately high relationship. The object-card correlates much lower with the event, however. All of the coefficients are low and positive, with the exception of one deviation toward definite negative relationship at age 13. The relation between picture and event narratives is somewhat higher than any of the others, particularly in the later years of the age range.

The coefficients of correlation between the interrogatories indicate, at practically every age, a positive but low relationship. The means run, as do those for the narratives, in increasing order of size: object-card with event, object-card with picture, picture with event.

When intelligence is 'partialled out,' the narrative coefficients are not significantly reduced, while the interrogatory coefficients suffer an average reduction of 0.11.

It remains only to present in Table III the relations between error scores in similar methods of reporting.

The coefficients of correlation between object-card and picture narratives are all positive and indicate mainly low relationship. Much the same is true for the correlation between object-card and event. The relations between picture and event narratives are, with one exception, low; three of them are negative.

TABLE III

CORRELATIONS BETWEEN REPORT SCORES: ERRORS

Age	Narrative			Interrogatory		
	Object-card Picture	Object-card Event	Picture Event	Object-card Picture	Object-card Event	Picture Event
9	0.227	0.412	0.169	0.408	0.344	0.390
10	0.264	0.203	−0.035	0.487	0.378	0.392
11	0.394	0.168	−0.081	0.413	0.084	0.412
12	0.352	0.595	−0.159	0.369	0.403	0.316
13	0.306	0.054	0.579	0.297	0.292	0.205
14	0.074	0.375	0.157	0.439	0.333	0.435
Mean	0.269	0.301	0.105	0.402	0.305	0.358

The correlation between interrogatory errors is uniformly higher than that between narrative errors, and indicates a definite relationship. There are few large deviations in the size of the coefficients at different ages, and the means may be taken as fair representatives of the relationships. In an increasing order of size these are: object-card with event, picture with event, object-card with picture.

The 'partialling out' of intelligence has a slight lowering effect on practically all of the coefficients, both for narrative and for interrogatory errors.

CONCLUSIONS

(1) The correlation between the narrative and the interrogatory forms of report upon the same material, measured in terms of items right, is positive but mainly low, with the amount of the relationship appearing as a function of the material upon which report is made. The correlation between the two forms of reporting, measured in terms of items wrong, is positive but negligible.

(2) When similar methods of reporting upon different report materials are intercorrelated, the following results appear: (a) Considering the narrative form of report, the relations between

object-card and picture, and between picture and event are positive and moderately high. The relation between object-card and event is negligible. (b) Considering the interrogatory form of report, the relationships between reports on the three materials are all positive but low, the means of the coefficients running in increasing order of size: object-card with event, object-card with picture, and picture with event.

(3) Narrative errors are related to a very small degree, but the relations are mainly positive. Interrogatory errors are related positively and to a moderate degree. They are more highly related than are narrative errors.

(4) The ability to report, in the light of the facts stated, seems to be largely a function of the material upon which the report is made and of the method of making the report. There is a small degree of correlation remaining between performances under these various conditions, even when intelligence differences are removed. The fact that relationships are so low can only indicate that the ability which has been measured in this experiment is largely a specific function, or otherwise stated, that the reliability of diverse report experiments is very low. Evaluation, therefore, of results upon sex and age differences, educability of report, and the like must carefully take into account this specificity of report ability both to methods and to materials.

INTELLIGENCE AND THE ABILITY TO REPORT

By JOHN A. McGEOCH, Washington University

Two investigations have indicated that the reports of children of normal intelligence are distinctly superior to those of children of subnormal intellectual status.[1] In one of these studies[2] it was found that the correlation between intelligence scores and correctness of report tended to be either zero or slightly inverse with a selected group of normal children and moderately positive with a selected group of *Ss* of equal chronological age but three years lower in mental age. The present study[3] presents the correlations between intelligence scores and various measures of ability to report on observed material, using as subjects the total population of each age from 9 to 14, found in the 4–8 grades in schools for supposedly 'normal' children, from which no selection save of the extremely subnormal children has been made. The report materials used were the Binet object-card, a picture, and an event.[4] The report scores which are in correlation with intelligence scores are the total number of right and wrong items and the percent of attestation. The intelligence measures are the National Intelligence Tests, Scale A, Form 1, and Army Alpha, Form 5, combined by adding algebraically their sigma standings and dividing by two.

In Table I are given the coefficients of correlation between items right and intelligence scores.

There are large gross differences between the coefficients at different ages and between those for different report materials at the same level. While these variations from age to age are too large to make a mean highly significant, the mean may serve as a convenient single expression of the tendency of the coefficients. Probably the most significant fact is that the means agree so closely in magnitude, thus indicating the presence of a small positive relationship between intelligence and correctness of report.

The coefficients of correlation between items right on the interrogatory and intelligence are given in Table II.

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[1] T. H. Pear and S. Wyatt, The testimony of normal and mentally defective children, *Brit. J. Psychol.*, 6, 1914, 387-419; J. A. McGeoch, The fidelity of report of normal and subnormal children, this JOURNAL, 36, 1925, 434-445.

[2] McGeoch, *op. cit.*

[3] From the Psychological Laboratory of the University of Chicago.

[4] These materials and the experimental methods used have been described by the writer, *loc. cit.* The number and intellectual status of the *Ss* are given in a preceding paper on age and sex differences, together with some slight variations in the experimental methods. *See* this JOURNAL, 40, 1928, 458-466.

TABLE I

CORRELATIONS: NARRATIVE, ITEMS RIGHT, WITH INTELLIGENCE

Age	Object-card		Picture		Event	
	r	P.E.	r	P.E.	r	P.E.
9	0.224	.066	0.254	.060	0.360	.069
10	0.258	.048	0.153	.054	0.268	.059
11	0.495	.042	0.177	.058	0.048	.057
12	0.397	.052	0.366	.056	0.271	.060
13	0.386	.068	0.342	.067	0.438	.066
14	0.035	.097	0.410	.080	0.380	.080
Mean	0.297		0.283		0.294	

TABLE II

CORRELATIONS: INTERROGATORY, ITEMS RIGHT, WITH INTELLIGENCE

Age	Object-card		Picture		Event	
	r	P.E.	r	P.E.	r	P.E.
9	0.705	.035	0.309	.057	0.296	.073
10	0.419	.043	0.436	.045	0.232	.061
11	0.334	.049	0.159	.059	0.228	.055
12	0.456	.049	0.339	.057	0.328	.057
13	0.353	.060	0.433	.061	0.459	.065
14	0.368	.082	0.241	.091	0.056	.095
Mean	0.439		0.319		0.266	

All of the coefficients are positive, and most of them are high enough to indicate a definite relationship. If we take the mean of all ages to represent the central tendency, the different materials yield correlations with intelligence in increasing order, thus: event, picture, object-card. It is clear that the degree of the relationship is a function of the material placed in correlation with intelligence and that, with the exception of the event, the interrogatory yields a higher correlation than does the narrative.

The coefficients of correlation between items wrong and intelligence for both narrative and interrogatory are shown in Table III, which omits the probable errors.

TABLE III

CORRELATIONS: ITEMS WRONG, NARRATIVE AND INTERROGATORY, WITH INTELLIGENCE

Age	Narrative			Interrogatory		
	Object-card	Picture	Event	Object-card	Picture	Event
9	−0.141	0.239	0.323	−0.415	−0.119	−0.260
10	−0.086	−0.005	0.258	−0.315	−0.180	−0.061
11	0.105	0.139	0.074	−0.471	−0.318	−0.230
12	0.121	0.115	0.149	−0.434	−0.379	−0.386
13	−0.116	0.084	−0.149	−0.263	−0.195	−0.305
14	−0.508	0.308	−0.277	−0.486	−0.055	−0.224
Mean	−0.104	0.146	0.063	−0.397	−0.207	−0.224

Both individual age coefficients and the means of all ages indicate a negligible relation between narrative errors and intelligence: the tendency with the object-card is slightly negative, that with the picture is slightly positive, and that with the event is positive in the earlier years of the age range and negative in the later years. Errors in interrogatory report bear a consistently negative relationship to intelligence; the amount of the relationship varies with the material upon which the questions are asked, in increasing order of magnitude, thus: picture, event, object-card.

The percentage of items attested represents the assurance of the *S* that his report is correct. This is, presumably, a factor different from those involved in the type of immediate memory involved in the making of a simple report upon observed data. It is particularly interesting, therefore, to know whether or not it is related to test intelligence. Table IV contains the correlations between test scores and percentages of attestation.

TABLE IV

CORRELATIONS BETWEEN ATTESTATION AND INTELLIGENCE

Age	Narrative			Interrogatory		
	Object-card	Picture	Event	Object-card	Picture	Event
9	−0.195	0.020	−0.043	0.087	−0.014	−0.104
10	−0.036	0.083	0.090	−0.078	−0.085	0.150
11	0.008	0.089	0.147	0.072	−0.111	−0.037
12	0.101	−0.056	−0.218	0.045	−0.162	−0.219
13	0.272	0.181	0.145	0.149	−0.153	0.177
14	−0.207	−0.148	0.191	−0.121	0.053	−0.092
Mean	−0.009	0.028	0.052	0.025	−0.078	−0.020

It is evident that there is no consistent positive or negative relationship throughout the different age levels, with the slight exceptions of the picture and event interrogatories. Means are hardly fair representatives here, but, whether we consider them or the individual coefficients at each age, it is clear that there is virtually zero relationship between percentage of attestation and intelligence, with both narrative and interrogatory forms of report. It is probable that we have in attestation a 'personality' factor. In any case, it is a trait not related to test-intelligence.

CONCLUSIONS

(1) Total items right on the narrative uniformly correlate positively, but slightly, with intelligence. Total items right on the interrogatory correlate with intelligence positively and somewhat more highly than do those on the narrative.

(2) Total errors in narrative report show no significant relation with intelligence. Interrogatory errors are correlated with intelligence negatively and to a low or moderate degree; the amount of the relationship is a function of the report material involved.

(3) Percentages attested are wholly unrelated to intelligence, no matter which of the report materials or of the reporting methods is considered. Attestation probably involves a 'personality' factor.

(4) It is clear that, in general, the relationship between intelligence and the ability to report is specific to three factors: the material upon which report is made, the method of making the report, and the method of scoring or measuring the report.

A STUDY OF EMOTIONAL SPECIFICITY

By David Wechsler and Harold E. Jones, University of California

The concepts of 'temperament' and 'emotionality' have appeared to assume a common factor or group of factors occurring in a great variety of emotional situations. This common or constitutional element is believed to produce a positive correlation between emotional responses, however stimulated and however measured. Clinical evidence has tended to support this view; such conditions as subthyroid lethargy, vagotonia, sympathicotonia, the manic and schizophrenic reaction types, and the abnormal irritability occurring in various forms of metabolic disturbance, are presented as illustrative of emotionality systems or levels.

Within a normal range, however, we have little certainty as to the extent to which general factors can be demonstrated. What is the relation between a person's 'anger' threshold and his 'fear' threshold? How specific are his anxieties and excitements? Unless an individual shows a certain consistency in his emotional reactions, we are obviously not justified in classifying him as emotional or unemotional; rather we would be constrained to denote his emotional tendencies separately with respect to specific contexts or situations.

Problem

The problem as here defined[1] was first presented by Wechsler in connection with his studies on the psychogalvanic reflex.[2] His method of approach was through an analysis of the intercorrelations obtained from a comparison of the galvanic responses of a variable number of *Ss* to a series of different emotion-provoking stimuli. The theory of this procedure was that, if emotionality was due to a single or general factor, an individual ought to show a consistency in his galvanic responsiveness irrespective of the type of stimulus used, whereas if it were due to a number of different factors specifically conditioned, individuals would show a corresponding degree of variable or specific reaction. The initial experiments supported the latter hypothesis. The correlations depended almost entirely upon the kind of stimuli used; they were high between galvanic reactions to stimuli evoking identical or similar affective states, and low or insignificant between psychogalvanic reflexes to stimuli evoking dissimilar affective states. Wechsler's conclusion, accordingly, was: "Individuals tend to react in a specific rather than in a general way to emotional situations." Due to limitations in the experimental data, this conclusion was advanced with some reserve.

*Accepted for publication December 20, 1927.

[1]This experiment was conducted in the Psychological Laboratory of Columbia University.

[2]D. Wechsler, Researches on the psychogalvanic reflex, *Arch. Psychol.*, 1925, no. 76; On the specificity of emotional reactions, this Journal, 36, 1925, 424-26.

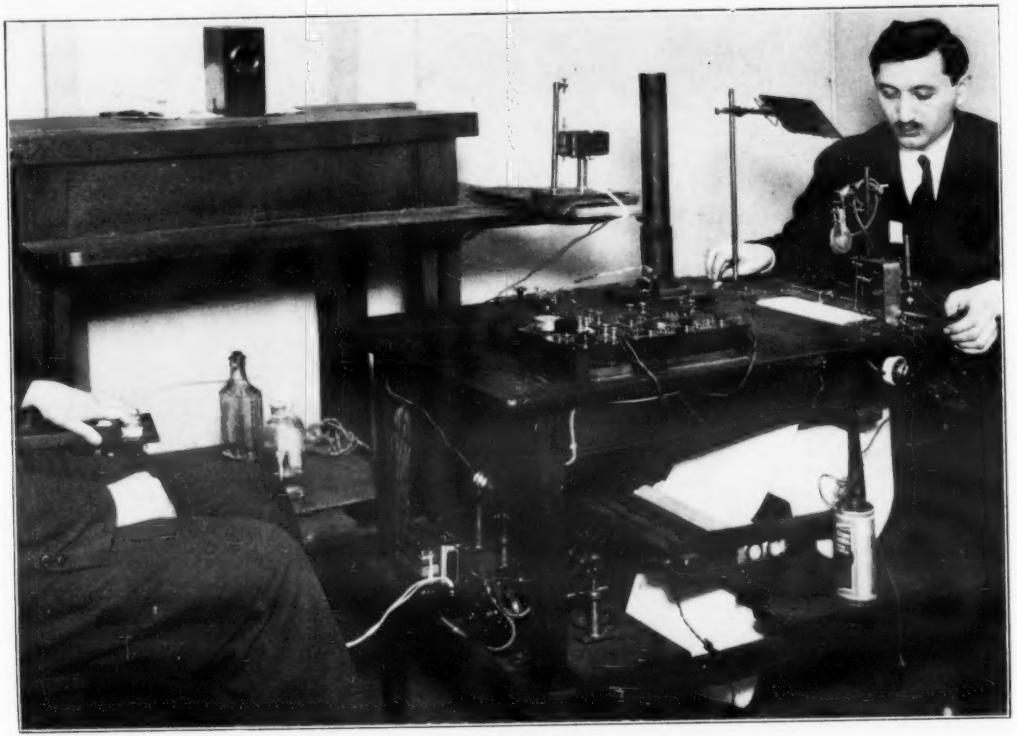

PLATE I

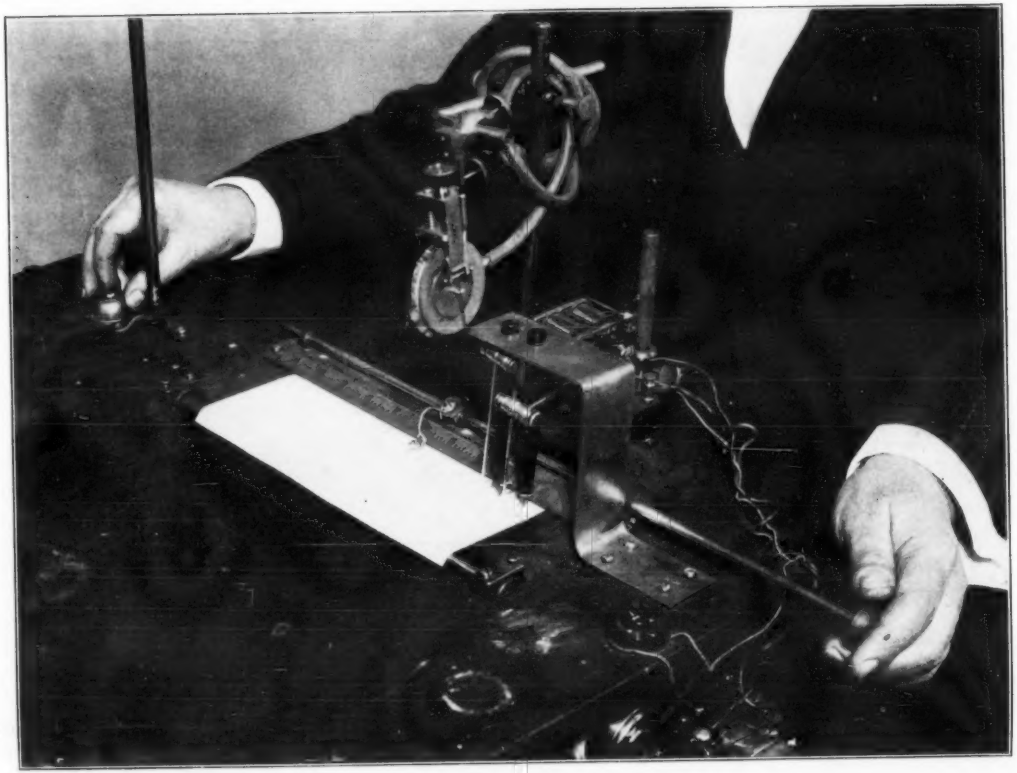

PLATE II

The object of our present investigation was to test out the position taken above and to obtain further data on the specificity problem. As in the cited experiments, our method of approach was through a study of self- and inter-correlations of the psychogalvanic response to emotionally stimulating situations.

Method and Procedure

Records were taken with 35 *Ss* (a random selection of college undergraduates, with a few graduate students) employing a standard series of 40 stimuli. The galvanic reactions were obtained by means of a D'Arsonval moving-coil type of galvanometer and exsomatic current, with the subject inserted as unknown resistance in the fourth arm of a Wheatstone bridge. A standard current of approximately 0.0001 M.A. was employed with every subject, variations of body resistance being compensated for through the use of a potentiometer. The electrodes were applied to the fingers of the hand, and consisted of small clay cups filled with normal saline, resting in somewhat larger cups of saturated $ZnSO_4$. Into the saline solutions the *S* was required to place the index and second finger of his right hand. The fingers were immersed the full length of the third phalanx, the surface exposed to the liquid being kept constant by the use of fitted rubber finger cots.

The galvanometric excursions were recorded by a mechanical device designed to give continuous graphic records of each subject's reactions. The essential features of the apparatus are apparent in the accompanying photograph, Plate I. The beam of light coming from the galvanometer is intercepted by a plane mirror which is so inclined as to reflect the beam perpendicularly down upon a moving surface of white paper. The paper is being displaced at a very slow speed over a narrow plate from a large roll of paper below. Above the paper a small ink-syphon is mounted on a horizontal bar (see Plate II) which moves laterally across and at right angles to the paper. The *E* observes the beam of light and, as it moves, follows it with the ink-syphon in the direction of the beam's displacement. In this way excellent graphs of the galvanometric excursions may be obtained, which compare favorably with direct photographic records. An advantage of this technique consists in the immediate visibility of the records, and in the opportunity to make accompanying notes on the same sheet. Further, the deflections can be magnified to a greater extent than in ordinary work with sensitive paper or film, since it is possible to use kymograph paper 9 in. in width. Errors, due to lag or slight deviations of the *E* in following the course of the light, are negligible save where very precise measurements of latent period are desired. Various electric markers can be readily attached, and two such are shown in the photograph, one connected with a metronome to mark seconds, and another to indicate moments of excitation.

The experimental situation was simplified as far as possible. The *S* was asked to seat himself in a chair equipped with an arm rest and electrodes. While he was making himself comfortable, he was shown how to keep his fingers in the saline-filled cups and given brief instructions as to what was to follow. The *S* had a general idea as to the nature of the experiment but knew nothing of its actual content. There were two *Es*, one who controlled the apparatus and another who gave instructions and presented the stimuli. The apparatus and the first *E* were at all times screened from the *S*.

TABLE I
CHARACTER AND ORDER OF STIMULI USED

Stimulus No.	Nature of Stimulus
1	Unexpected noise (turning on of motor).
2	Verbal Instructions: "I am going to read you some numbers. When I am through I want you to repeat them backwards."
3–6	Four series of digits to be repeated backwards.
7	Verbal Instructions: "I am going to show you some pictures on a screen. As soon as you see each picture say out loud the first word that comes to your mind."
8–11	Four colored pictures thrown on a Trans-Lux screen by means of an opaque projector. The pictures were advertisements from the *Saturday Evening Post*. Although chosen for a supposed provocativeness, their content proved to have little emotional value.
12	Verbal instructions: "Now I am going to say some words. You are to respond to each word by telling me the first word that comes into your mind."
13–29	Seventeen association words (one buffer and sixteen taken from Whateley Smith's series).[1] Eight of the words were chosen for minimal emotional value, and eight because of their position at the head of Smith's list. The former we termed "non-critical," the latter "critical." The two classes of words were given in alternate groups of 4 each.
30–32	Physical aggression: *E* approached *S* unexpectedly and grasped his nose between thumb and forefinger. Repeated once, and the third time merely simulated.
33–35	Loud auditory stimulus (klaxon sounded).
36	Adjustment of inductorium electrodes to *S*'s leg.
37	Verbal instructions: "Now I am going to give you an electric shock. At first it will be mild, but later it will be very severe. When you begin to feel the current say 'feel' and when you can't stand it any longer say 'stop.'"
38	Auditory stimulus (buzz of make-break on inductorium).
39	Inductorium shock. *S* reports 'feel.'
40	Inductorium shock. *S* reports 'stop.'

The above stimuli were given without undue interruption, and so far as possible in the same manner, to all *S*s. Where the standard conditions could not be adhered to, the records were thrown out; this occurred in the

[1] W. Whateley Smith, *The Measurement of Emotion*, 1922, 76.

case of 6 *Ss* (due to failure to follow instructions, interruptions from outside, or apparatus defects). The duration of an experiment varied from 20–30 min.; this variation was largely due to the extra time that had to be allowed to certain *Ss* between successive stimulations, owing to the character of their galvanic reactions. Thus, although we planned to allow only 20 sec. between the first and second digits tests, it was sometimes necessary to wait 20 or 30 sec. additional, owing to the occurrence of spontaneous or multiple reactions to the initial stimulus. In a sense, however, it might be said that the interval between stimuli was physiologically standardized, since it was determined by the time required for the achievement of stability in each individual's resistance.

The reactions of the *S* were thus recorded as a continuous graph 6 or 7 feet in length, presenting a large number of undulations, the successive 'waves' of which represented the responses to the various stimuli in order of presentation (together with secondary responses.) The deflections were measured to the nearest millimeter and recorded in their proper place on a 40-fold table covering the total number of stimuli employed. Owing, however, to the fact that stimuli 36–40 (electric shock) were hard to control and in sensitive *Ss* often produced deflections beyond the edge of the paper, the results for these last 4 stimuli were not used in our calculations. The final data available from our experiments thus consisted of the psychogalvanic reactions of 29 adult male *Ss*, to a series of 36 distinct stimuli standardized for the given conditions of the experiment, and quantitatively comparable.

Results

Our first step in the analysis of the data was to calculate individual correlations between galvanic reactions to any given stimulus with any other series in the series. We calculated 112 such correlations using the rank-order method (ρ converted into r).[4] The correlations were then grouped into 3 classes: (1) self-correlations, those between repeated or identical stimuli, *e.g.* between the first and second startle evoked by a klaxon; (2) intercorrelations between responses to similar stimuli, *e.g.* association words by pictures, or the sound of starting motor by sound of klaxon; (3) intercorrelations between responses to dissimilar stimuli, *e.g.* word association by aggression stimuli. The values for each group of coefficients were then averaged and compared, both as to magnitude and reliability.

Table II

Self-Correlations

Stimuli	r	P.E.
Mental effort (Digit repetition stimuli 3,5 x 4,6)	0.81	0.05
Aggression (Stimuli 30 x 31)	0.80	0.05
Startle (First by second klaxon)	0.67	0.07
Average	0.76	

[4]The writers are indebted to Mr. Herbert S. Conrad for this statistical work.

The reliability of the single items, or pairs of items, in tests of motor skill, memory or intellectual ability is seldom as high as the average found above. Yet such traits or abilities are commonly considered more stable and more subject to precise measurement than the emotional reactions with which we are concerned. Applying the Spearman-Brown formula, $r_{nn} = n \cdot r_{xy} / [1 + (n-1) r_{xy}]$, the reliability of our psychogalvanic reflex measures for the total of 4 mental effort stimuli becomes 0.90; the reliability of the 3 responses to aggression is 0.92, and of the 3 startle responses, 0.83. This is almost certainly an exaggeration, however, for (1) the rapid 'exhaustion' of the psychogalvanic reflex when the same stimuli are repeated, cuts down the variability of the later measures, and hence reduces self-correlation; and (2) the first and second responses, given at any one time, may be supported by a temporary mood condition, yielding a higher correlation than if they were separated by a day or more. With due regard to these considerations, however, the consistency of the results remains higher than had been expected.

TABLE III

SELF-CORRELATION IN THE ASSOCIATION TESTS[5]

Stimuli	r	P.E.
Critical words (1st four x 2nd four)	0.55	0.08
Non-critical words (1st four x 2nd four)	0.53	0.09
Pictures (1st and 3rd x 2nd and 4th)	0.44	0.10
Average	0.53	

The absolute deflections and variability of the deflections to these stimuli are considerably less than are the responses to the mental effort, aggression and startle stimuli just discussed. Not a few *Ss* gave zero deflections to the uncritical words and pictures coming at or near the end of the series. As might be expected, the degree of self-correlation appears to vary with the emotional intensity of the stimulus.

The series of digits, the pictures, and the association words are similar in that they present to the *S* a situation, more or less simple, requiring the formulation of a verbal response. The emotional involvement is most marked when conflicts interfere with a prompt formulation. The sound of the electric motor and the klaxon warning, are also similar to each other in that they are loud and unexpected.

[5]The association test data will be reported in more detail in a separate paper.

TABLE IV

INTERCORRELATIONS OF DEFLECTIONS TO SIMILAR STIMULI

Stimuli	r	P.E.
Word by picture association (Stimuli 8-11 x 13-29)	0.56	0.08
Word association by mental effort (3-6 x 13-29)	0.46	0.09
Picture association by mental effort (3-6 x 8-11)	0.55	0.08
Verbal directions (2 x 7)	0.25	0.11
(2 x 12)	0.43	0.09
(7 x 12)	0.68	0.08
Startle (motor by klaxon) (1 x 33-35)	0.46	0.09
Average	0.48	

TABLE V

INTERCORRELATIONS FOR DISSIMILAR STIMULI

Stimuli	r	P.E.
Startle (motor) by mental effort (1 x 3-6)	0.21	0.12
by picture association (1 x 8-11)	0.24	0.12
by word association (1 x 13-29)	0.03	0.12
by aggression (1 x 30-32)	0.15	0.12
Mental effort by aggression (3-6 x 30-32)	0.04	0.12
by startle (klaxon) (3-6 x 33-35)	0.13	0.12
Picture association by aggression (8-11 x 30-32)	0.21	0.12
by startle (klaxon) (8-11 x 33-35)	0.35	0.11
Word association by aggression (13-29 x 30-32)	0.11	0.12
by startle (klaxon) (13-29 x 33-35)	0.47	0.10
Aggression by startle (klaxon) (30-32 x 33-35)	0.44	0.10
Average	0.22	

The coefficients run considerably lower than in the previous groups, and the P.E.s are of course higher. Only two coefficients are more than four times their P.E.s. The higher correlation between aggression and startle, if due to other than chance features, might be considered as the outcome of a certain similarity of the stimuli; both are of a primitive and provocative character. The correlation between klaxon and word association cannot be attributed to this, but perhaps rather to the fact that in these two classes of stimuli the decrement due to exhaustion of the psychogalvanic reflex is more conspicuous than elsewhere: with the klaxon because of its intensity, and with the word association because of the extended number of stimuli used. Hence the correlation would be expected to be reënforced by individual differences in the rate of negative adaptation.

The predictive value of each class of stimuli for other classes is indicated by the average of the intercorrelations with each of the other classes. For the 5 principal groups under discussion these are as follows:

TABLE VI

THE PREDICTIVE VALUE OF STIMULI

Stimuli	r	k
Startle (klaxon) (33–35)	0.37	0.93
Word association (13–29)	0.33	0.94
Picture association (8–11)	0.32	0.95
Mental effort (3–6)	0.29	0.96
Startle (motor) (1)	0.22	0.97

The predictive value of dissimilar stimuli for each other is thus seen to be extremely low. Of the classes considered, the three sounds of the klaxon have the highest predictive value, but the differences possess slight reliability and in any case the coefficient of alienation is so great that the interpredictive functions can have no value.

SUMMARY

Correlations between psychogalvanic reflex deflections can be classified into three groups, the coefficients constituting a hierarchy with but little overlapping.

Self-correlations between repeated stimuli (*e.g.* two identical sounds) give an average of 0.76 (Table II).

Inter-correlations between stimuli imposing similar tasks (*e.g.* associations to two words, or to a word and a picture) average 0.50 (Tables III and IV).

Dissimilar stimuli (*e.g.* a loud sound *vs.* an association word) yield very low coefficients, averaging 0.22 (Table V). Correction for attenuation, if proper data were available, would raise cross correlations from 0.05–0.15 points. But under the conditions of the experiment, even the corrected intercorrelations would leave the coefficients of alienation from 0.80 upwards, indicating a high degree of specificity in the galvanic response.

Our data agree very closely with previous results obtained by Wechsler. The question that still remains is whether a similar specificity would be shown by other physiological indicators of emotion.

GALVANOMETRIC TECHNIQUE IN STUDIES OF ASSOCIATION

By Harold E. Jones and David Wechsler, University of California

During the past two decades recurrent use has been made of the psychogalvanic response in attempts to measure the affective values of words, in the detection of 'guilt,' and in clinical studies of emotional complexes.[1] The major portion of this work has been of a tentative and exploratory nature, with estimates of the method ranging from the very confident claims of Whately Smith[2] to opinions less wholly enthusiastic.[3] It would appear that progress has been slow, and that technical difficulties have discouraged many psychologists from continuing in this field.

It is of course true that the psychogalvanic response involves a delicate and complex procedure, with many variable factors having to do with the electrodes, the E.M.F., the area and location of the skin contacts, the coil suspension, and the processes of recording the deflections. Irregularities or lack of standardization at any one of these points may result in grave disturbance of the data. In the present report, the writers are concerned not with matters of apparatus technique which have been held as constant as possible,[4] but with the effect of alterations in the schedule of stimuli, and the influence of sequence and temporal position in series.

The stimuli employed were the following association words, given in the order shown with one half of our *Ss*,[5] and in the reverse order with the remaining half: table, work, carrot, white, money, afraid, dance, woman, glass, flower, swim, pencil, insult, marry, love, kiss.

The stimuli were derived from Whately Smith's list of 100 words, arranged by him in a rank order according to a weighted mean psychogalvanic response obtained in an association experiment with 50 *Ss*. Eight of the words (kiss, love, marry, insult, woman, dance, afraid, money) were taken from the 12 standing at the head of his list, and showing average deflections ranging—in terms of his recording system—from 72.8 to 35.6.

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*Accepted for publication November 18, 1927.

[1] This paper includes results reported in part at the 1926 meeting of the American Psychological Association, and abstracted in the *Psychol. Bull.*, 24, 1927, 173-174.

[2] "I believe it will be be clear to any reader . . . that this phenomenon [the psychogalvanic response] is by far the most delicate, reliable, and quantitatively accurate method at present known for detecting and measuring emotional changes." Whately Smith, *The Measurement of Emotion*, 1922, 25.

[3] Cf., for example, J. A. Larson, The cardio-pneumo-psychogram and its use in the study of the emotions, with practical application, *J. Exper. Psychol.*, 5, 1922, 324.

[4] The writers' apparatus has been described in the preceding article *supra*, 601.

[5] The *Ss* were a random selection of male undergraduate and graduate students of Columbia University. Thirty-five *Ss* were tested, the reversed order of stimuli being used with every other *S*. Due to interruptions, or errors of technique, the total number of cases was reduced to 29 for the later statistical treatment.

The remaining 8 (pencil, swim, flower, glass, white, carrot, work, table) were chosen from the 12 at the bottom of the list, with deflections from 18.5–14.2. In subsequent discussion, words from the top of the list, having conspicuous emotional values, will be called 'critical,' while the others will be called 'indifferent' or 'non-critical.' The order of stimuli indicated in the above list, beginning with table, will be called A; the reverse order, given to alternate *Ss*, will be called B. In all cases the association words were preceded by a buffer word, 'salt;' the total series occurred in the middle of a stimulus schedule, the first part of which consisted of a picture association test and a test of repeating digits backward. When the association words were reached in this schedule, instructions were given: "Now I am going to say some words. You are to respond to each word by telling me the first word that comes into your mind." The stimulus words were given orally, separated by an interval (from 10–30 sec.) sufficient to permit the resistance of the *S* to return to its previous level after the psychogalvanic response. As each word was sounded, the *E* pressed a key which gave a stimulus signal on the continuous kymograph record. A second *E* (concealed from the *S*) tended the apparatus and made observations on qualitative aspects of the reactions. A reproduction of two representative responses by the same *S*, is given in Fig. 1.[6]

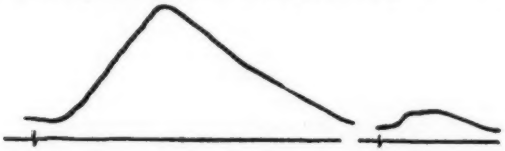

FIG. 1. REPRODUCTION OF TWO REPRESENTATIVE RESPONSES FROM THE SAME *S*

S: "afraid" R: "not"

S: "flower" R: "beautiful"

The deflections to each stimulus word were measured to the nearest mm. and measures of central tendency were computed. In Table I the stimulus words are arranged in the order of magnitude of the Whately Smith means for the same words, and the comparative data of the two investigations are listed.

The gross magnitudes obtained are of course relative to the arbitrary standard conditions of the experiment.[7] The two lists agree in the demarcation of critical and non-critical words, and in placing the sex words conspicuously at the top of the list. A correlation of 0.90 was obtained between the two rankings on the basis of the means, and of 0.95 when the second list is ranked on the basis of the medians. Another important comparison is in what Smith terms the resolvability or range of variability of the psychogalvanic response. The mean value of the word eliciting the largest deflection in Smith's list is 72.8, while the mean value at the other end is only one fifth as great. In the writers' data this ratio becomes about 3.5 to 1, but remains greater than the resolvability of reaction time (2.5 to 1) for the same words. This may be considered to be one of the advantages which the psychogalvanic response shows over reaction time, or for that matter over most of the laboratory indicators of emotion. Its wider relative variability allows for greater precision of measurement, and tends to increase the reliability of the results.

[6]Owing to its length, an entire record cannot conveniently be reproduced here.

[7]The scale on which the deflections occur depends upon the reflecting system used, the E.M.F., the original sensitivity of the galvanometer, and the shunt.

Table I gives the average deflection for each word, combining the two orders A and B. If, however, we compute averages separately for the two orders of presentation, a striking difference appears.

TABLE I

SHOWING THE MEAN DEFLECTION FOR EVERY STIMULUS WORD OBTAINED BY SMITH, AND THE MEAN AND MEDIAN DEFLECTION OBTAINED IN THIS EXPERIMENT

Stimulus-word	Smith: Mean deflection	This Experiment: Mean deflection	This Experiment: Median deflection
kiss	72.8	11.46±1.04	9.50±1.30
love	59.5	10.69±0.96	8.33±1.19
marry	58.5	10.39±0.97	8.67±1.22
woman	40.3	7.96±0.92	7.67±1.15
dance	37.4	7.31±0.60	6.67±0.77
afraid	36.8	9.12±1.23	7.00±1.55
money	35.6	7.58±0.69	5.00±0.86
insult	32.5	8.65±0.75	7.50±0.94
table	18.5	4.23±0.54	4.00±0.68
work	18.3	6.85±1.10	4.50±1.37
carrot	18.2	6.70±1.20	3.33±1.50
white	17.8	4.69±0.60	3.00±0.77
glass	17.6	5.31±0.70	4.60±0.88
flower	16.5	6.12±0.72	5.00±0.90
pencil	15.4	4.65±0.60	3.25±0.77
swim	14.2	3.38±0.33	2.60±0.41

TABLE II

SHOWING THE MEDIAN DEFLECTION FOR THE TWO ORDERS IN WHICH THE STIMULUS-WORDS WERE PRESENTED

Stimulus-word	Order of presentation	A Median	Order of presentation	B Median
table	1	5.25±1.06	16	0.72±0.81
work	2	7.75±1.25	15	0.93±2.24
carrot	3	6.50±2.12	14	3.17±2.11
white	4	5.25±1.06	13	0.81±1.17
money	5	4.75±1.03	12	5.50±1.31
afraid	6	7.75±2.18	11	6.50±1.15
dance	7	7.25±1.11	10	6.25±1.05
woman	8	7.75±0.42	9	7.50±1.73
glass	9	4.88±1.41	8	2.50±0.99
flower	10	4.50±0.84	7	5.25±1.55
swim	11	1.50±0.39	6	2.88±0.87
pencil	12	1.50±0.93	5	3.83±1.25
insult	13	6.50±1.01	4	8.50±1.63
marry	14	8.25±1.32	3	10.50±2.00
love	15	8.75±1.90	2	7.50±1.37
kiss	16	8.75±1.32	1	10.50±2.14

Inspection of Table II immediately suggests the importance of position in series. The effect of adaptation is obvious, in spite of the fact that an attempt was made to minimize this by employing a buffer word, and by giving the association test in the middle of a much larger schedule of stimuli. The median[8] deflection for 'table' is 5.25 when it appears first in the list, and 0.72 when it appears last. The median deflection for 'work' is 7.75 when it appears second in the list, and only 0.93 when it is given next to the last. It will be observed, however, that in the case of the most 'critical' words the emotional value is not greatly negatived by a late position in the series. The words 'kiss,' 'love' and 'marry' give the highest median deflections whether they appear first or last in the list. In the A order the 'non-critical' words are given positions 1-4 and 9-12 in the list; the mean of the 8 medians is 4.64. The less favorably situated 'critical' words (positions 5-9 and 13-16) yield a mean of 7.47. If now we change to order B, the mean for the 'non-critical' medians drops nearly one half to 2.51, while the mean of the 'critical' words increases slightly (and unreliably) to 7.82. It is, then, established that the factor of position in series exerts a greater influence upon the words of slight emotional tone than upon those which already have a significant affective value. This relation is further emphasized in Table III.

Several of the above differences, particularly for the 'critical' words, have a very low reliability; nevertheless, of 16 differences, 13 are in favor of the earlier position in series, and the exceptions, in two of the cases at any rate, appear to be reversals due to the influence of emotional carry-over from contiguous words. Thus, the word 'glass' when it occurs 8th in series has a smaller deflection than when it occurs 9th, but in the latter case it occupies a post-critical position, following the word 'woman.'

The influence of position in series may be brought out in another manner. In the group of *S*s taking the A order of words, each individual's average deflection was computed for the 8 'critical' and for the 8 'non-critical' words; the correlation between these two variables was 0.61 (rho corrected to r). For the *S*s given the B order of words a practically identical correlation (0.64) was obtained. If now we consider the total group and correlate the critical by the non-critical averages, a similar r should be found, unless some disturbing influence has affected the comparability of the two groups. The obtained r, 0.23, may be considered as evidence for such a disturbing influence. But the sole difference in technique with the groups was in the order

[8] In these and in further comparisons, we have made use of the median deflection for each word rather than the mean; the median more genuinely represents the distribution, and is freer from the influence of aberrant individual responses.

of presentation of the words; the effect of which was to give large deflections of the 'non-critical' words when they occurred first, and small or insignificant deflections when they were given in the middle or at the end of the series.

TABLE III

SHOWING THE IMPORTANCE OF THE POSITION OF THE STIMULUS-WORDS IN THE STIMULUS-SERIES

	Stimulus-word	Change in position	Deflection difference in favor of earlier position	% of increase, per unit of change in position
Non-critical	table	16 to 1	4.53	41.9
	work	15 to 2	6.82	56.4
	carrot	14 to 3	3.33	9.5
	white	13 to 4	4.44	61.7
	pencil	12 to 5	2.33	22.2
	swim	11 to 6	1.38	18.4
	flower	10 to 7	.75	5.5
	glass	9 to 8	−2.38	−48.7
		Mean	2.65	20.8
Critical	kiss	16 to 1	1.75	1.3
	love	15 to 2	−1.25	− 1.1
	marry	14 to 3	2.25	2.4
	insult	13 to 4	2.00	3.4
	money	12 to 5	− .75	− 1.9
	afraid	11 to 6	1.25	3.9
	dance	10 to 7	1.00	5.3
	woman	9 to 8	.25	.33
		Mean	.81	2.1

Table III should be read: The word 'table' shows a median deflection 4.53 mm. greater when it is presented first in series than when it is presented sixteenth. Taking as a base the median value when it is presented last, this value is increased 41.9% for each unit of change in forward position, or a total of 628% for a change from sixteenth to first.

A question may arise as to the equivalence of groups A and B in the functions measured. The mean of the 16 medians is 6.09 for group A, and 5.18 for group B. From Table II it is obvious that this difference is due to the fact that in group A the 'non-critical' words have priority. A fairer basis of comparison would be with the middle 8 words rather than with the total series; here the means are respectively 4.99 and 5.03, and we may accept this as an indication that the two groups were comparable, for the purposes of this experiment.

CONCLUSIONS

(1) In a serial association experiment, the magnitude of the psychogalvanic response varies with the temporal position of the stimulus. Hence, in clinical studies or in the detection

of guilty knowledge, the emotional values of stimulus words cannot be compared without taking into account the factor of position in series. This of course might be inferred from the general experience with the rapid 'exhaustion' of the psychogalvanic response in repetitions of the same stimulus, but the application of the principle to association experiments has frequently been neglected. Such an error may account, in part, for the inconsistent results occasionally reported in the use of the psychogalvanic response.

(2) 'Critical' and 'non-critical' words cannot readily be distinguished unless a preceding series of at least five buffer words has been used.

(3) The importance of position is greater with 'non-critical' than with 'critical' words; stimuli connected with complexes yield reactions of considerable uniformity, regardless of where they occur.

(4) If different *S*s are to be compared as to emotivity, the schedule of presentation can be varied only within very narrow limits.

(5) When reduced to a comparable scale, the median deflection values for each of the 16 words are closely similar to the results obtained by a previous experimenter. In terms of averages, this indicates a high degree of dependability of the psychogalvanic response.

AN INVESTIGATION OF BLUFFING

By Ernst Thelin and Paul C. Scott, University of Cincinnati

In a report of an experiment conducted in one of his classes at the University of Pennsylvania. Professor S. W. Fernberger has raised the question of the prevalence of bluffing by students.[1] We are told that "a class of 29 showed evidence of more than the usual bluffing in a quiz. A trap was set for them in the next examination in an attempt to determine just how much and how frequently the college student would bluff. To this end, the class was asked to discuss ten terms, devoting not less than one page to each. The seventh term in the list was made up out of whole cloth—'*psychoterminality.*' Of the group of 29 men and women, only 2 made the positive statement that they did not know what the term meant. Six other left the question blank without a definite negative statement. The other 21 students made some sort of an answer ranging from one-half to three pages in length."

At the time of the appearance of the above report, an investigation not greatly dissimilar in character was being conducted at the University of Cincinnati. The general problem of this investigation was to study the existence and extent of bluffing among university students. The investigation did not concern itself at all with any moral question involved and no effort was made either to defend bluffing as honest or to condemn it as dishonest.

For clarity and specificity of discussion, some definition of the term 'bluffing' was felt to be necessary. Hence a working definition might be stated as follows: Pretending to have greater knowledge than is actually possessed.

As a vehicle for the investigation, there was constructed a battery of tests in which there were provided opportunities for bluffing, as defined above. Since English is a universal high school and university requirement and a subject of which all students are expected to have a general knowledge, it was decided to give the test the appearance of being a general English test. As a basis it was decided to use a number of items selected from the Cross[2] and the Wakefield English Tests.[3] Their objective form was observed in the construction of all our tests, which in general took the forms of the New Type Tests. Several new test forms, however, were used, *e.g.* the Vocabulary Tests.

Situations conducive to bluffing were secured by inserting irrelevant and fictitious items into tests containing legitimate items. These irrelevant and fictitious items were arranged in such a way that they would not easily be detected in a survey of the material of the test. An attempt was made to form the fictitious items without any basis of fact. The legitimate items ranged from those of common to those of less general familiarity; the fictitious items, from those apparently easy to those of extreme difficulty. The extent to which the person taking the test expressed familiarity with items of this fictitious character was scored as bluffing.

The following excerpts will give an idea of the test in its final form, after revisions based upon preliminary administrations to about 100 university students and 47 high school students.

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*Accepted for publication September 17, 1927.

[1] S. W. Fernberger, On bluffing in examinations, this Journal, 38, 1927, 155 f. Cf. also H. B. English, *ibid.*, 40, 1928, 350.

[2] E. A. Cross, *Cross English Test for High Schools and Colleges*, 1923.

[3] E. W. Scott, *Wakefield Diagnostic English Test for Secondary School Pupils*, 1926.

ENGLISH TEST

General instructions. This is a general English test. Some of its items are so easy that no one should miss them. Some are so difficult that probably no one will get them all right. Accordingly, every individual's score will have a relative value. Your score will indicate whether you approximate the average of your class, or whether you exceed that average, or fall below it.

If you wish, you may glance through the whole test before you begin work. The test has been timed, so you should experience no difficulty in completing it during the class period. The various sections of the test should be taken in the order in which they appear.

Idiomatic expressions. Direction: Draw a heavy line under the correct form in each sentence.

(1) They were not unmindful (of, to) the French customs.

[There were 10 such sentences, all legitimate.]

Recognizing a sentence. Directions: Some of the following sentences are faulty, and some are correct. Print an (F) before each sentence that is faulty; before each that is correct print a (C).

(1) The reason I was absent was because I was ill.

[Ten such sentences, all legitimate, were presented.]

Shakespeare—A. Directions: Indicate with an (H) or an (M) whether each of the following statements relate to Hamlet or Macbeth.

(1) Malcolm suspected Macduff of treachery.

(2) Mercutio was loyal to the Thane of Cawdor.

[Twenty statements were presented, 12 legitimate, 8 fictitious.]

Shakespeare—B. Directions: Underline the character to whom you ascribe each of the following quotations:

Group I. Quotations from Hamlet.

(1) To be or not to be; that is the question (Claudius, Hamlet, Horatio).

(2) Call me what instrument you will, though you fret me, you cannot play upon me (Ophelia, Laertes, Rosencrantz).

[Five quotations were presented, 2 genuine, 3 fictitious.]

Group II. Quotations from Macbeth.

(1) The night is long that never finds the day (Lady Malcolm, Mercutio, Oliver).

[Five quotations, all fictitious.]

Novels—A. Directions: Underline the proper title or author as found in each parenthesis.

(1) A Tale of Two Cities by (Sir Walter Scott, Ralph Connor, Charles Dickens).

(2) Paths of Victory by (Edith Wharton, Jules Verne, John Erskine).

[Fifteen titles or authors, 12 genuine, 3 fictitious.]

Novels—B. Directions: Within the parentheses before each title, write the number of the author of that work, as given in the second column.

Group I.

[Five titles and five authors, all genuine.]

Group II.

[Five titles, two genuine; and five authors, two genuine.]

Group III.

[Five titles and five authors, none genuine.]

Literature. Directions: Indicate, by underscoring, the appropriate item or items in each of the following.

(1) To whom is literature most indebted for the presentation of the immortal figure of Faust? (Marlowe, Schiller, Goethe).

(2) From which of the following ancient writers is Milton supposed to have taken many of his classic figures in his L'Allegro and Il Penseroso? (Archides, Plymander, Harmonides).

[Ten questions, 5 genuinely related to the answers suggested, 5 with fictitious answers suggested.]

Vocabulary test—A. Directions: A student employs more than 4000 words of the English language adequately to express his thoughts. What is the extent of your vocabulary? Four thousand words, 6000, or even more? In the following tests, words have been selected from groups of graduated word lists on the basis of which we shall be able with some accuracy to determine the breadth of your vocabulary.

[Fifteen synonyms, antonyms, or words standing in some other relation to a given word, were to be underlined among those printed in parentheses. In six cases the words were coined.]

Vocabulary test—B.

[In 10 sentences, the person tested had to underline among words in parentheses a certain type of word, *e.g.* archaic, concrete, hackneyed. In 5 of the sentences one or more of the suggested words were coined.]

Vocabulary test—C.

[Paragraphs were presented with certain words underlined, some genuine, some coined, and the person tested was asked to underline for each of these words its synonym among three words in parentheses. Some of the words in parentheses were also coined.]

The items of the test were by their nature such as to lend themselves to the following system of scoring: (1) all legitimate items were subject either to correct or incorrect checking; (2) every fictitious item marked or accepted for definition was counted as a bluff; (3) in any particular situation where there was more than one possible correct answer, or opportunity for multiple bluffing, each correct answer and each bluff counted as one. The total number of legitimate items was 120, the irrelevant and fictitious items totaled 113.

In order to secure a comparison of the results of the test with the intelligence rating of the students, they were given the Otis Self-Administering Tests of Mental Ability (Higher Examination: Form A, For High Schools and Colleges). A further comparison was made of the test score of each student with his Average Grade, representing the average of all grades received while in the university. Since grades were found expressed in qualitative forms, in order to facilitate statistical treatment they were given quantitative values as follows: A = 1; B = 2; C = 3; D = 4; E = 5.

The test was given, in various classes of the College of Liberal Arts, to 291 students classified as follows: 19 freshmen, 138 sophomores, 75 juniors, 47 seniors, 12 specials. To get a basis of comparison between the amount of bluffing done by university students and by those engaged in various vocations outside of student life, the test was given also to groups outside of the University. These groups totaled 58, and included bookkeepers, toolmakers, chauffeurs, pressmen, salesmen, superintendents, and managers of a laundry company.

Results

Universality of bluffing. The investigation seems definitely to have established the existence of bluffing among university students, at least among those groups to which the test was given. Some bluffing was done by every student; the amount of bluffing varying from just below 5% to more than 81%. That the amount of bluffing in general was quite large

is indicated by Table I. Half of the students bluffed 46.58% or more; a fourth of them bluffed 60.97% or more, and only a fourth of them bluffed 30.55% or less. The average bluffing score of all students was 44.6%.

The same condition, however, was not found to exist in the groups to whom the test was given outside of the University. The amount of bluffing by these groups ranged from 0 to 69.1%, the mean of the entire group being 25.7%; the median 19.16%; and the Upper and Lower Quartiles 38.44% and 6.78% respectively. Of the 58 people in this group, 4—a bookkeeper, a salesman, and managers of two local laundries—or 7%, did no bluffing at all, *i.e.* they did not express familiarity with any of the 113 fictitious items. The extreme cautiousness of these 4 is further attested by the narrow range of the legitimate items which they attempted. This narrower range of items attempted by all the members of the groups outside the University, which may have been due to unfamiliarity with the nature of the material which constituted the body of the test, resulted in lower *C* (Correct) scores. They attempted less, hence their *C* scores and *B* scores are both smaller. But even a comparison of the average *C* and *B* scores of the two groups (Table II) shows that the University group did proportionately slightly more bluffing than did the group outside of the University, the proportions being 0.873 and 0.829.

TABLE I

DISTRIBUTION SHOWING PERCENT OF BLUFFING

Percent of Bluffing	Students		Outside Group	
	No.	%	No.	%
80–84	1	.34	0	—
75–79	0	—	0	—
70–74	2	.68	0	—
65–69	23	7.96	1	1.72
60–64	58	19.93	1	1.72
55–59	19	6.53	4	6.89
50–54	22	7.56	2	3.44
45–49	30	10.30	3	5.17
40–44	15	5.15	1	1.72
35–39	26	8.93	8	13.79
30–34	25	8.59	2	3.44
25–29	19	6.52	1	1.72
20–24	30	10.30	5	8.62
15–19	11	3.78	6	10.37
10–14	3	1.03	5	8.66
5–9	6	2.06	7	12.06
0–4	1	.34	12	20.68
Totals	291	100.00	58	100.00
Mean	44.6		25.7	
Median	46.58		19.16	
Upper Quartile	60.97		38.44	
Lower Quartile	30.55		6.78	

Age and bluffing. To determine whether or not there existed any relationship between age and bluffing, the data were given the treatment shown in Table II, which gives the average age by classes of the various groups with number and percent of legitimate items answered correctly; number and percent of fictitious items attempted; intelligence test scores; and average grades. The freshmen are shown to have bluffed most (50.9%), the seniors least (39.4%), with both sophomore (40.3%) and junior (45.4%) groups bluffing less than the freshmen and more than the seniors. That this relationship between class and amount of bluffing was not consistent,

however, is shown by the fact that the sophomores bluffed slightly less than the juniors. Moreover, the average age of the sophomores was slightly less than that of the freshmen. The average of the special students (46%) was larger than that of the sophomores, juniors, or seniors but less than that of the freshmen. The coefficient of correlation between age and amount of bluffing was found to be -0.1697 ± 0.38, which would suggest a slight tendency toward decreased bluffing with increasing maturity.

Sex and bluffing. Our data further lent themselves to a treatment designed to show the relative amount of bluffing by men and women. This treatment of the data is given in Table III.

TABLE II

AVERAGES BY CLASSES AND GROUPS

Group	Age	No. *C*	No. *B*	% *C*	% *B*	I.T. Sc.	Av. Grade
Freshmen	19.6	67.5	57.5	56.1	50.9	58.6	3.18
Sophomores	19.4	66.7	45.6	55.4	40.3	58.2	2.85
Juniors	20.4	65.7	51.7	54.6	45.4	58.6	2.70
Seniors	21.5	64.9	44.9	54.0	39.4	57.8	2.60
Specials	28.7	68.4	52.2	56.8	46.0	57.9	2.23
WholeUniversity Group	20.4	65.4	50.4	54.5	44.6	58.1	2.78
Non-University Group	26.3	36.9	32.8	31.0	25.7		

An examination of Table III shows that the men in the Freshmen class and those classified as Specials bluffed considerably more than did the women of those groups, the ratio being 63.1 to 38.6% for freshmen and 59.2 to 33.2% for the special students. The women of the sophomore, junior and senior groups, however, bluffed more than did the men of those groups, the ratios being for the sophomores 46 to 34.7%, for the juniors 46 to 44.8%, and the seniors 39.9 to 39.2%. For the junior and senior groups the differences are small, and are probably not statistically significant. The average bluffing score of all the men (45.8%) was slightly more than that of the women (43.4%).

TABLE III

AVERAGES OF MEN AND WOMEN BY CLASSES

Classes	Sex	Age	No. *C*	No. *B*	% *C*	% *B*	I.T. Sc.	Av. Gr.
Freshmen	M	20.2	68.8	71.2	57.3	63.1	63.6	3.34
	F	19.1	66.2	43.8	55.2	38.6	53.7	3.03
Sophomores	M	19.5	68.6	39.1	57.2	34.7	58.5	2.82
	F	19.4	64.8	52.2	53.8	46.0	58.0	2.89
Juniors	M	20.4	66.5	50.9	55.3	44.8	59.4	2.72
	F	20.5	65.0	52.6	54.1	46.0	57.9	2.69
Seniors	M	21.7	67.4	44.8	56.2	39.2	58.1	2.84
	F	21.4	62.5	45.1	52.1	39.9	57.6	2.37
Specials	M	29.0	71.0	67.0	59.1	59.2	65.0	2.25
	F	28.4	65.9	37.5	54.8	33.2	50.8	2.22
Totals	M	20.3	66.3	51.8	57.0	45.8	59.0	2.82
	F	20.5	64.6	49.0	54.0	43.4	57.2	2.74

Relation between bluffing and grades and between bluffing and intelligence test scores. Two questions attained commanding interest as the investigation progressed and it became apparent that students were giving varying degrees of expression of familiarity with fictitious items. First, is there any relation between the amount of bluffing and grades earned at the University, and vice versa? Secondly, is there any relation between the amount of bluffing and intelligence, *i.e.* do the students who bluff most, score highest in intelligence tests? In order to answer the first, the data were tabulated so as to show the amount of bluffing on the basis of grades; to answer the second they are tabulated on the basis of scores received in the Otis Intelligence Test.

The first treatment (Table IV) shows that students who got highest grades (2.99 or above) did least bluffing; answered correctly a slightly higher percentage of the legitimate items; and scored highest in the Intelligence test. The correlation between amount of bluffing and grades, $-0.09 \pm .039$, however, was so small as to be statistically insignificant.

TABLE IV

DISTRIBUTION OF PERCENT CORRECT, PERCENT BLUFF, AND INTELLIGENCE TEST SCORE ON A BASIS OF AVERAGE GRADES

Average Grades	No.	% *C*	% *B*	I.T.Sc
1 to 1.99	28	57.5	43.5	63.4
2 to 2.99	106	54.3	42.0	58.0
3 to 3.99	86	52.9	47.6	56.6
4 to 4.99	12	54.4	47.6	52.0

TABLE V

DISTRIBUTION OF PERCENT CORRECT, PERCENT BLUFF, AND AVERAGE GRADES, ON A BASIS OF INTELLIGENCE TEST SCORE

Intelligence Test Score	No.	% *C*	% *B*	Av. Gr.
70 or above	16	59.1	40.6	2.56
60–69	93	57.2	47.2	2.45
50–59	89	53.1	44.6	2.81
40–49	28	49.6	38.0	2.97
30–39	5	52.2	45.8	3.69
20–29	1	51.6	40.7	2.42

The second treatment (Table V) showed that those who scored high in the intelligence test (60 or above) answered the highest percentage of the legitimate items correctly; received higher grades; and averaged slightly more in bluffing. The 16, however, who scored highest in the intelligence test (70 or above) averaged less in bluffing than did those scoring less (with the exception of those scoring between 40 and 50); while those who scored between 60 and 70 in the intelligence test averaged most in bluffing. As in the case of the relationship between bluffing and grades, however, the correlation between bluffing and intelligence test score ($0.0756 \pm .043$) was so small as to be of no statistical significance.

Thus it would appear, on the face of the results, that it is not the student who bluffs who gets highest grades, and that those students who bluff most are not those of highest intelligence, but those just above the average in intelligence.

SUMMARY

The findings of the investigation may be summarized as follows:

(1) Among the university groups to which the test was given, bluffing was found to be universal. That the extent of this bluffing was impressive

is indicated by the fact that 50% of the students expressed familiarity with 46% or more of the 113 fictitious items. The percentage frequency with which fictitious items were checked as being familiar ranged from 5% to more than 81%.

(2) Among the groups outside of the university, bluffing was found to be much less prevalent only about 17% expressing familiarity with 46% or more of the fictitious items. The median percentage score of bluffing for these groups was slightly above 19%, the scores ranging from 0 to 69.1%. Seven percent of those comprising these groups checked none of the fictitious items as being familiar to them.

(3) In proportion to the number of legitimate items checked correctly, the amount of bluffing by university students was but slightly larger than that by the non-university groups. In other words, the latter groups not only refrained from expressing familiarity with fictitious items, but practiced the same inhibition with reference to legitimate items with which they had no familiarity.

(4) The data show a negative correlation between amount of bluffing and age. Thus maturity, and all it implies, appears to function to decrease the type of activity under investigation.

(5) Although the relative amount of bluffing was not constant for the various classes of the university, on the average the men were found to bluff more extensively than the women. This condition was most marked in the case of the freshmen, the percentage frequencies of bluffing for that group being, men 63.1, women 38.6.

(6) The students who ranked highest academically averaged lowest scores in bluffing in the test. In other words, the investigation appears to indicate that the highest grades, at least, are not secured by bluffers.

(7) Students who bluffed most were those scoring just above the average in the intelligence test, and not those scoring highest.

SEASHORE'S MEASURES OF MUSICAL ABILITY APPLIED TO CHILDREN OF THE PRE-SCHOOL AGE

By ESTHER MCGINNIS, University of Minnesota.

The purpose of this investigation[1] was to determine whether it is possible to use Seashore's measures of musical ability[2] with children of the pre-school level; to determine what methods could be devised for so doing, and what reliability and validity the results of such an adaptation would have.

It would be of interest and value to parents and educators if it were possible to detect differences in musical ability in young children. Such information would help in planning for the musical education and training of the child and would also throw light on the question of the innateness of musical ability. Seashore in his *Psychology of Musical Talent* says the ability to distinguish pitch differences, and the ability to distinguish intensity differences are innate and not modifiable.[3] No experimental verification of this statement, however, could be found in the literature on children below school age; neither is there any available data on the effect of training given to very young children who appear to lack musical ability.

Individual differences in children's reactions to music are observable very early. Babies move their bodies rhythmically in time to strongly accented music; older children 'do what the music says' by stamping to loud music, tiptoeing to soft; some children 'carry a tune' and show a keen delight and pleasure in hearing music. Others do none of these things. Why? Is it because of physical differences in ability to discriminate rhythm, intensity, pitch, etc., and are these abilities innately determined? In a nursery school situation, should the child who fails to respond to the music hour be excused because of inability, or may the lack of response be due to social conditioning while the innate ability is present but needs help in order to be expressed? To determine the presence or absence of musical ability becomes an important question in the treatment of the young child at home and in the nursery school.

As a first approach to the study of this problem, three of the six Seashore tests were selected for trial: intensity, pitch and consonance. The others are probably equally important, but those named are less complicated and involve distinction between two sounds only. Each record was given individually twice to each of 16 children, 8 boys and 8 girls, enrolled in the nursery school at the Institute of Child Welfare, University of Minnesota. The developmental status of the group is indicated in Table I.

The first difficulty to be met is the length of the task. Each side of a record includes 50 pairs of notes requiring 50 judgments. Few young children are able to give sustained attention for so long a period. By marking the record immediately after the last note of the first 30 judgments with a slight scratch of a pin point, each side of the record may be given in two parts. There is enough space between the end of the thirtieth and the

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[1]From the Institute of Child Welfare. Made under the direction and with the assistance of Dr. Florence L. Goodenough.

[2]C. E. Seashore, *The Psychology of Musical Talent*, 1919; *Manual of Instructions and Interpretations for Measures of Musical Talent*, 1919; *A Survey of Musical Talent in the Public Schools*, 1920.

[3]*Op. cit.*, 59 ff., 99 ff.

beginning of the thirty-first judgment, so that the needle can be placed on the scratch, the victrola started, and the drag in speed will be taken up by the time the first of the next pair of notes is reached.

The next difficulty is the inability of the child to understand Seashore's terms "weak and strong," "high and low," "better and worse." The words "loud and soft" (used in music hour in the nursery school) "baby and daddy" referring to the story of the Baby bear (examiner's voice very high) and the Daddy bear (examiner's voice low and deep), and "pretty and ugly" were substituted. Practice was given with a mouth organ and with the victrola until the child seemed to comprehend clearly what was wanted.

TABLE I

RANGE AND AVERAGE IN MONTHS OF THE GROUP'S CHRONOLOGICAL AND MENTAL AGES, TOGETHER WITH THE RANGE AND AVERAGE OF THEIR INTELLIGENCE QUOTIENTS

Measure	Chronological age	Mental age	Intelligence quotient
Range	41–59	42–79	97–148
Average	49	59	122

The third difficulty is the uninteresting nature of the material of the tests to the young child. The sounds are monotonous and unmusical and the children were bored and disappointed, as music to them means melody and action. To overcome this very important difficulty several means of motivation were used.

The tests were called games. Intensity was the loud and soft game, and it was compared to a similar game used in the nursery school. Pitch was the baby bear and daddy bear's voices, and the game was to tell who spoke the last note, the baby bear or the daddy? Consonance was the pretty and ugly game. The mouth organ, used at first only by the experimenter for illustration, later (properly sterilized each time) was given to the child, and at the beginning and end of the experiment he was allowed to play it. A game was made of this, the examiner guessing which notes the child was playing. The record blank was filled out directly in front of the child so he could see the spaces fill up. The children were interested in the amount done, wanted to see how much other children had finished, and pointed with pride to what they had accomplished. At times, when the children wanted to stop or had a tendency to answer before they heard the second of a pair of notes, the examiner beat the notes in the air with her hand, making no noise but holding the child's attention.

TABLE II

AVERAGES OF OMITTED RESPONSES

Trial	Test		
	Intensity	Pitch	Consonance
1	6.8	6.9	0.25
2	4.9	5.7	0.37

The fourth difficulty is omitted responses. The responses were scored on a percentage basis by taking the percent right of the answers, not of the total. If too many omitted responses occurred, that section of the record was repeated. The number of omitted responses decreased on the average from the first to second trial in intensity and pitch. See Table II.

The small number of omitted responses in the consonance records is due to the longer time between judgments. It would greatly improve the other records for use with young children if the time between judgments were increased. The correlation of omitted responses on both trials with C.A. is −0.494, showing a tendency for the older children to make fewer

omissions.[4] The correlation of omitted responses with mental age is −0.54. Correlation of omitted responses in pitch with those in intensity is only +0.148 which shows that individual children were erratic in their omitted responses and varied from test to test.

TABLE III

RANGE OF THE PERCENT CORRECT JUDGMENTS IN THE BEST TRIAL, CORRELATION OF PERFORMANCE WITH C.A. AND WITH M.A., AND NUMBER OF CHILDREN GIVING RECORDS COMPARABLE TO THE NORMS OBTAINED BY SEASHORE FOR 5TH GRADE CHILDREN

Test	Range percent correct in best trial	Correlation of performance with		No. children to be compared with Seashore's 5th grade norms
		C.A.	M.A.	
Intensity	51.2–65.3	+0.14	+0.20	6 above 55% right
Pitch	50.5–62.2	−0.08	−0.30	9 above 55% right
Consonance	39.6–68.0	−0.10	−0.26	9 above 58% right
Combined score			−0.18	

In each test a number of children made a ranking sufficiently high to be compared to Seashore's norms for 5th grade children. The low correlations with C.A. and M.A., given in Table III, show that the ranks made on these tests do not depend on chronological or mental age.

The correlation between consonance and pitch is the highest, that for intensity and pitch second, and that for consonance and intensity lowest in both sets of data.

TABLE IV

CORRELATIONS BETWEEN EVERY TWO OF THE THREE TESTS (Using best trial)

Test	Our results	McCarthy's results*
Intensity and Pitch	+0.492	+0.09 to +0.38
Consonance and Pitch	+0.623	+0.21 to +0.48
Consonance and Intensity	+0.26	+0.10 to +0.16

*From data about to be published by Dorothea McCarthy, University of Minnesota, on the reliability and validity of the Seashore Music Tests. The tests were given to groups of school children, junior high and college students. Validity was studied by comparison with ratings and school marks in music.

The results for each test were analyzed by columns to see whether the percent right decreased in order of the difficulty of the judgments. While no such tendency could be observed, the fact may have been obscured by the erratic attention of the children. Frequently a child did as well or better on the most difficult column as on the easiest.

The reliability for the intensity record compares favorably with the results of McCarthy and others. That for pitch is very low, which is probably due to the erratic responses of the children rather than to an inability to grasp the meaning of the test, as it compared favorably with the other tests in validity. Consonance is also fairly reliable.

Three of the nursery school teachers independently rated each of the children on general musical ability.[5]

[4] All correlations are Spearman's rank order unless otherwise stated.

[5] The coöperation of Miss Marion Mattson, Miss Catherine Thompson and Miss Adelia Boynton, of the Child Welfare Institute, University of Minnesota, is gratefully acknowledged.

A composite of the three ratings was made by averaging the three ranks for each child. The correlation of the composite rank with C.A. is +0.176 and with M.A. +0.147, showing practically no relationship. Agreement on individual children was high. On the composite rank the first five were in the first half of the group, and the lowest four were among the last six on every rating.

The results obtained by McCarthy are given for comparison in Table VIII.

TABLE V

RELIABILITY COEFFICIENTS BASED UPON THE CORRELATION* BETWEEN FIRST AND SECOND TRIALS OF EACH TEST

Coefficient	Test		
	Intensity	Pitch	Consonance
Our results	+0.43 P.E. = .14	−0.02 P.E. = .17	+0.37 P.E. = .15
McCarthy	+0.68	+0.70	

*Pearsonian correlation.

TABLE VI

CORRELATION BETWEEN RATINGS

Rating	Correlation	Multiple Correlation	
1 and 2	+0.66	1.23	+0.72
1 and 3	+0.55	2.13	+0.66
2 and 3	+0.43	3.21	+0.55

TABLE VII

CORRELATIONS BETWEEN SCORES IN THE THREE TESTS AND COMPOSITE AND INDIVIDUAL RATINGS

Rating	Test			Composite score in the three tests
	Intensity	Pitch	Consonance	
1	+0.04	+0.20	+0.39	+0.40
2	−0.13	+0.21	+0.21	+0.13
3	+0.13	+0.25	+0.41	+0.40
Composite	+0.04	+0.17	+0.42	+0.37

TABLE VIII

MCCARTHY'S CORRELATIONS BETWEEN SELF ESTIMATES, MUSICAL TRAINING, OR MUSIC GRADES WITH SCORES ON TESTS IN TERMS OF PERCENTILE RANK

Ss	Test		
	Intensity	Pitch	Consonance
58 adults, 2 groups	+0.40+0.54	+0.54+0.68	not given
35 adults	+0.16	+0.47	+0.34
71 5th and 6th grade children	+0.46	+0.50	+0.52
81 Junior high school students	+0.85	+0.32	+0.29

In the case of the consonance test the coefficients obtained compare favorably with the findings for older children and adults. The correlation between the composite scores on the three tests and the composite of the three ratings (+0.37) is quite as high as was ordinarily obtained between teacher's judgments of intelligence and scores earned on the earlier revisions of the Binet Scale. With suitable modifications, particularly in the direction of shortening the records and increasing the intervals between judgments, it appears probable that these tests might become valuable research instruments for use with young children.

THE FUSION OF NON-MUSICAL INTERVALS

By E. R. Guthrie and H. Morrill, University of Washington

In a recent number of this Journal Brues published the results of an investigation of the fusion of non-musical intervals.[1] A recent experiment in the University of Washington Laboratory resulted in a striking confirmation of his results by the use of a radically different method.

Brues used Stern variators to sound two notes. One of the notes was of constant pitch and the second note was varied so as to include twenty-four quarter-tone intervals from the first tone to its octave. Four *Os* were asked to record the degree of fusion of the combination on a scale from 1 to 5 (5 representing perfect fusion). For most of the tone combinations 10 judgments were obtained from each *O*. For a portion of the octave 6 additional judgments were made. The average degrees of fusion reported are represented on the accompanying Fig. 1 by the small circles. Brues' averages have been reduced to S.D. units and on the graph are plotted from the mean of the Washington data with standard deviations equated.

In the Washington experiment Stern variators were also used. Forty-four intervals were explored, extending from unison (395 with 395 v.s.) to an interval beyond the fifth, 395 v.s. with 617 v.s. at intervals of approximately 5 vibrations per sec. Calibration of the Stern whistles by comparison with accurate tuning forks and reeds showed a virtually linear error in the readings of the variators. The pitches as indicated on the variators extended from 400–615 v.s. at intervals of 5 v.s. Under the conditions of the experiment, which included a necessity for rapid setting of the instruments, there was an average deviation of 0.84 v.s. from the true pitch as determined by tuning forks.

Os in groups of about 50 were asked to record each combination as 'pleasant' or as 'unpleasant.' The tone combinations were given in random order. When this had been done, the same *Os* were asked to judge, on a repetition of the series, whether or not each combination had 'consonance' or 'dissonance.' With 5 groups the 'pleasant-unpleasant' discrimination was obtained first. With 3 groups the 'consonance-dissonance' discrimination was made first.

Consonance had not been discussed in the elementary psychology class of which the *Os* were members. It was described to them as a combination of purity, smoothness, and good-blending—a description which may be revolting to many psychologists, but which proved satisfactory to naïve *Os*.

Reports on the discrimination of 'consonance' and 'dissonance' were obtained from 381 *Os*, and on 'pleasantness' and 'unpleasantness' from 372 *Os*. The percentage of the 381 *Os* that reported any tone combination as having 'consonance' is indicated by the solid line in Fig. 6. The percentage of the 372 *Os* that reported each tone combination as 'pleasant' is indicated by the dotted line.

It will be noticed that there is no tendency for the high points of 'consonance' or of 'pleasantness' to coincide with the intervals of simple ratio, or with the 'musical' intervals of the equally tempered scale whose positions are indicated by the second, fourth, sixth, eighth, etc., of the circles indicating Brues' data. The degrees of fusion indicated in the Washington experiment and in the experiment of Brues are in substantial agreement when differences in method are taken into account. In addition to using

*Accepted for publication February 14, 1928.

[1] A. M. Brues, The fusion of non-musical intervals, this Journal, 38, 1927, 624-638.

a different method of recording judgments, Brues worked with tones of another pitch range, from 256 v.s to 512 v.s.

We do not suggest that 'unpleasantness' and 'consonance' coincide to the extent indicated in the curves. The results indicate only that the term 'consonance' as described, and the term 'pleasantness' led to very similar responses from the *Os*. The two curves have, indeed, a difference which is far beyond the range of chance, the probability that the 'pleasantness' curve is a chance variant of the 'consonance' curve being 4.2×10^{-8}.

The curves obtained have a slight resemblance to the theoretical curves of consonance worked out by Helmholtz for tones including several partials. They have no resemblance to the curve indicated by Helmholtz for combinations of pure tones, although Dr. S. H. Anderson of the local physics department reported that the variators showed only very faint traces of the third partial, others being inaudible.

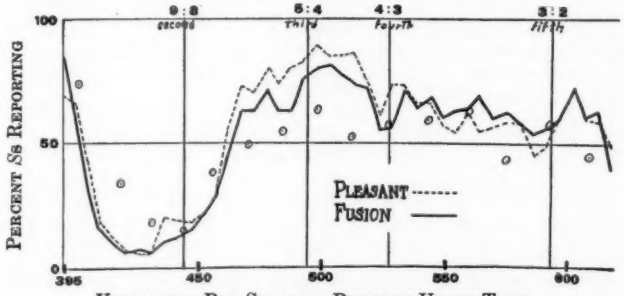

FIG. 1

Pitch combination	% reporting consonance	% reporting pleasantness	Pitch combination	% reporting consonance	% reporting pleasantness
395–395	86	70	395–508	77	85
395–400	58	66	395–513	74	86
395–405	32	42	395–518	72	75
395–410	16	18	395–523	55	61
395–415	10	12	395–528	58	73
395–421	6	7	395–533	71	73
395–426	7	6	395–538	64	66
395–431	6	7	395–544	68	65
395–436	10	20	395–549	60	57
395–441	12	19	395–554	63	54
395–447	15	19	395–559	64	62
395–452	21	22	395–564	59	54
395–457	29	36	395–569	60	56
395–462	45	54	395–575	62	58
395–467	63	78	395–580	58	58
395–472	63	76	395–586	54	44
395–478	71	80	395–591	65	48
395–482	63	74	395–596	69	58
395–487	63	80	395–602	72	70
395–492	76	83	395–607	59	59
395–498	80	89	395–612	62	58
395–503	81	85	395–617	38	48

TWO COMPARISON STIMULI WITH ONE INITIAL STANDARD IN LIFTED-WEIGHTS

By WILBUR S. HULIN, Princeton University, and L. R. FRAZIER, Harvard University

Lifted-weight experiments have been "the classical field for absolute impression."[1] Absolute impression is based on the stimulus error.[2] Recently Fernberger and Rudisell have shown that the stimulus attitude can yield psychometric functions of high precision,[3] a conclusion which suggests the possibility that the persistent factor of absolute impression might also conform to the psychometric function.

TABLE I

SHOWING THE MEASURES OF PRECISION (hs), THE LIMENS, THE INTERVALS OF UNCERTAINTY, AND THE VALUES OF SUBJECTIVE EQUALITY FOR THE NORMAL AND TEST SERIES

O	Series	h 'greater'	h 'less'	Limen 'greater'	Limen 'less'	Interval of uncertainty	S.J.E.*
	Normal series	.0728	.0770	100.3436	91.6700	8.6736	96.0068
H	Test series, 1st comp	.0913	.1007	97.0880	91.8316	5.2564	94.4595
	Test series, 2nd comp.	.0913	.0879	99.3834	90.0124	9.3760	94.7004
	Normal series	.0894	.0829	100.9780	89.4996	11.2984	95.3609
N	Test series, 1st comp.	.1612	.0757	97.7884	89.0312	8.7572	95.7070
	Test series, 2nd comp.	.0574	.0757	99.2768	91.3976	7.8792	94.7903
	Normal series	.0949	.0769	98.4892	87.7884	10.7048	93.6973
S	Test series, 1st. comp.	.0561	.0316	108.9132	69.4708	39.4424	94.8412
	Test series, 2nd comp.	.0414	.0824	112.0024	81.8552	30.1472	91.1300

*The 'subjective judgment of equality' is represented by Xi, the intersection of the limens.

This possibility was tested with lifted weights by obtaining judgments on two successive comparison stimuli following a single initial standard.[4] The method of constant stimuli was used with three *O*s (H, N, and S).[5] The standard weight was 100 grm. H judged 5 comparison weights of 88, 92, 96, 100, and 104 grm. N and S judged 7 comparison weights, adding 84 and 108 grm. Each *O* first judged a normal series of standard and single comparison. In the test series the standard was presented and followed by a comparison weight upon which the judgment 'greater,' 'equal,' or 'less' was immediately reported, and then directly after this judgment the second comparison weight was presented and immediately judged, also

*Accepted for publication January 17, 1928.

[1] E. B. Titchener, *Experimental Psychology, Instructor's Manual, Quantitative*, 1905, 231. See references to Martin and Müller, Kinnaman, Fröbes, *ibid.*, 205 ff., 304 ff.

[2] *Ibid.*, lxiii-lxvii, 203 f.

[3] S. W. Fernberger, An experimental study of the 'stimulus-error,' *J. Exper. Psychol.* 4, 1921, 76; E. S. Rudisell, Constancy of attitude in weight perception, this JOURNAL, 36, 1925, 587.

[4] Method suggested by Professor C. C. Pratt, Harvard University, in 1923.

[5] Apparatus and procedure similar to Fernberger's *op. cit.*, 66.

in terms of the initial standard weight. In the normal series each *O* gave 200 judgments apiece on each comparison weight; in the test series H gave 200 judgments on each weight, and N and S each gave 100 judgments apiece on each weight. The results were computed by Urban's tables. 'Equal' judgments were discarded. The total of 6200 judgments is summarized in Table I.

Table I shows a significant similarity between the psychometric functions of the test series and normal series for H and N. S gave many 'equal' judgments and his results are fairly negative. In the test series, all *h*s for H and one *h* for N are higher than in the normal series; one 'interval of uncertainty' for H and both for N are smaller than in the test series; and one of the values of 'subjective equality' for N and both for H are farther from the value of the objective standard than in the normal series.

APPARATUS

Instrument for Observing Ocular Movements

By S. M. Newhall, Yale University

The instrument was devised to facilitate direct observation of eye-movements for certain problems where photographic or other elaborate technique would be either unwarranted or inexpedient.[1] It improves conditions of observation principally by presenting *E* with an enlarged illuminated image of *S*'s eye and by controlling in some measure *S*'s head movements and visual stimuli.[2] In its original use in counting the saccadic movements of children, there were also the requirements of simple adjustment, freedom from restraint or intimidation, and ready portability. The manner of meeting the requirements seems evident in the description below.

In Plate I the device is shown clamped to a laboratory standard which in turn is clamped to a table. *S* looks at the stimulus material exposed in the rack, R. At the same time he holds the portion of his upper lip just below his nose against the middle of the rod, S. This steadies his head but does not interfere with verbal reports. The open space adjacent to the rod is to allow free breathing.

E, at E, looks through an observation tube, T, which he trains upon *S*'s right eye-ball from the side without intercepting *S*'s line of vision. Training is achieved with a slightly braked universal joint upon which the tube carrier, C, is mounted. The 20-diopter convex lens set in the end of the tube can be seen in the figure. Focussing is accomplished by sliding the tube back and forth in the carrier. When the distance between eye and lens is somewhat less than its focal length, a virtual erect image of the eye which fills the field can be obtained. With practice *E* can quickly train and focus with one hand. In doing this, the writer has found it convenient to place forefinger and thumb on the end of the tube near his eye at E with the other fingers resting on the tube carrier. Forefinger and thumb then serve in focussing and all the fingers in training, without shift of position. There is a hand-wheel, visible below C, which can be used for horizontal training; but it is really unnecessary and, indeed, cannot be used for the combined one-hand adjustment. The knurled head on the right side of the tube carrier simply allows tightening to prevent the tube sliding out during transport.

Local illumination for both *S* and *E* is secured from a 60-watt full-frosted Mazda lamp, L. It is mounted as shown so that emitted light clears the top of the observation tube to the stimulus material and also passes the end of the tube to *S*'s eye. The source is too far to the side to be seriously distracting. Furthermore, if the light is switched on and off only while *S* is in position, the danger of dazzling him is eliminated. *E* is himself protected by the square screen represented behind the lamp.

Since some of the space relations required study, actual dimensions may be useful and these are here given in terms of cm. Baseboard A (40 x 13 x 1.2) is rigidly screwed to baseboard B (65 x 8 x 2) at an angle of 55°. To

[1] The instrument is due to Professor R. Dodge in principle.

[2] W. Miles has recently reviewed the earlier simple lens and mirror methods, including contributions by Erdmann and Dodge, Öhrwall, Javal, Landolt, and Freeman. He also has described an ingenious peep-hole technique. Cf. *J. General Psychol.*, 1, 1928, 373 f.

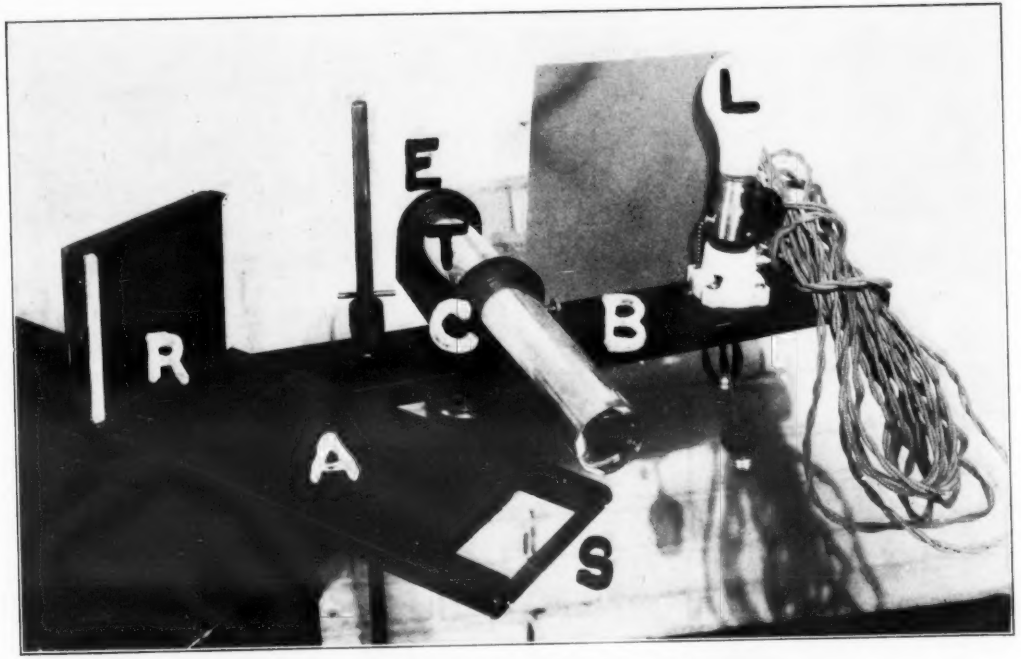

PLATE I. INSTRUMENT FOR OBSERVING OCULAR MOVEMENTS

A are screwed two parts, *i.e.* the half-round brass rod jaw or head-rest, S(1.2 diam.) and the stimulus rack, R (13 x 13 x 2), the edges of which are painted dead white to avoid contrast with the exposure card. The distance SR is 30 cm. Mounted on B are the lamp, L, and the tube carrier, C (base, 10 x 7 x 1.2; ends, 7 x 7 x 1.2). The angle formed by the lines LS and SR is 80°. The universal mountings of the tube carrier is a hinge attached to a vertical shaft which passes through a hole in baseboard B. The hinge permits up-down and the shaft left-right movement. Sliding easily through holes in the ends of the carrier is the thin-walled, internally blackened, brass tube, T (30 x 4.5 diam.). The vertical distance between the top of baseboard B and the axis of the tube is 6 cms. The mountings of the tube carrier is so located on B that the tube will point at about right angles to B when trained on *S*'s eye. Under these circumstances the end of the tube somewhat overlaps the right side of the head-rest as shown in Plate I. If binocular exposure is desired, it is obviously important that in training the tube it must not be allowed to overlap so much as to interrupt the line of vision of the right eye.

Several further points relating to design or use seem worthy of mention. One problem was to make the observation tube short enough to provide *E* with as large a retinal image as feasible but long enough to avoid distraction of *S* by over-proximity of *E*. Thirty cm. seemed a reasonably satisfactory dimension. The fact that the dull black interior of the tube restricts the field to *S*'s eye favors concentration in observing that object. The same condition facilitates a simple check of head movements. When the image of *S*'s eye and lid as a whole moves, in *E*'s restricted field, the movement is readily noticed and it must be due to head movement.

Experience indicates that a special technique is desirable in counting reading movements. If *E* attempts to follow with his own eye *S*'s movements, observation becomes difficult or impossible. The writer obtained best results by fixating a relatively stationary point as near as possible to the exposed area of the eye-ball (edge of lid) while attending to a fixed point on the moving ball itself (vein or outer edge of iris). If the reading material is difficult regressions are likely to occur and sometimes they cannot be distinguished from the long swing of the eye to the left in starting the next line. *E* may have an assistant who, with *S* reading aloud, follows the text and taps lightly with a pencil at the end of each line. Thus *E* is made confident of the ending of each line and of the time to record the number of movements observed during the reading of that line. The assistant could, if desired, take the total reading time with a stop-watch. He could also alternate functions with the *E* as a check on personal equation.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Theory of Imagination in Classical and Mediaeval Thought. By M. W. BUNDY. University of Illinois Studies, Vol. 12, Nos. 2-3. Urbana, University of Illinois Press, 1927. Pp. 289.

Attacked from many angles and on many grounds, the terms 'image' and 'imagination' have maintained a precarious foothold in psychology for the last quarter of a century. At present writing, with the epithets 'eidetiker' and 'introvert' in the air, only the hardier dare avow the persistence in our daily thinking of the imaginal taken in its more flagrant forms. Yet the most scrupulous of objectivists, after carefully expunging the offending terms from his professional diction, must admit a grain of curiosity as to the sources of the strange delusion which the words connote. This curiosity Professor Bundy's scholarly and readable narrative is eminently calculated both to whet and to allay. Planned originally as an excursion into critical sources, in the interests of the time honored literary controversy over the respective provinces of fancy and imagination, the study expanded in the author's hands, taking on new objectives. The volume issued is announced as the first installment only of a comprehensive survey of theories of imagination and fancy, from the dawn of Greek thought to the present. Thought is a unity, asserts the author in the Preface; the history of critical and esthetic terms must inevitably be bound up with the contemporaneous conclusions of epistemologist, metaphysician, and psychologist. Hence the rational procedure of the critic is simply to allow representatives of successive ages to speak for themselves; explaining their utterances, not in the light of his own theory but as having come from certain schools of thought, from certain personalities, at certain times; thereby revealing the drift of certain great traditions.

In Bundy's *Theory of Imagination in Classical and Mediaeval Thought* the psychologist may therefore trace not merely the varying fortunes of the two terms in question (*εἰκασία* from the verb *εἰκάζω*, to copy—later *imaginato*—and *φαντασια*, from the verb *φαίνω* to appear, or *φαντάζω*, to render apparent). He will find also a lucid and fairly leisurely account of descriptive psychology itself in the making; from the first tentative differentiation of the 'existential' and the 'experimental' in Pre-Socratic fragments, through the painstaking piling up of observation, the gradual clarification of the boundaries between stimulus and sensation and image, at the hands of classical philosophers and medieval schoolmen; a story told in large part through the medium of copious quotations from the works of Plato, Aristotle, Epictetus, Philostratus, Augustine, Aquinas, Dante (to mention only the more prominent sources); the whole linked by a lucid running commentary by the author, who furnishes also a final and definitive summary. Especially significant from the point of view of the esthetician is the evidence (gathered from many quarters) of the gradual re-infiltration of the terms *εἰκασία* and *φαντασία* into esthetic criticism, from which they had been rigorously excluded by Aristotle, on metaphysical rather than critical premises. Excerpts from the works of Dante, illustrative of the final synthesis of medieval theories of the function of the imagination, in Chapter XI, are perhaps of interest to the literary critic rather than to the psychologist.

Much of the material presented, it is true, is available in other sources, in the works of Croce, Saintsbury, Bosanquet, Ambrosi, Schweitzer, and in translations and histories of psychology and philosophy. Nowhere, however, is it accessible in such compact yet comprehensive form, subject to so little distortion at the hands of critic or translator; with the original text of significant passages bracketed in the translation.

In this wealth of material, representing over fifteen centuries of meticulous observation, the psychologist of an inquisitive turn may find more or less clearly adumbrated the majority of the skirmishes and squabbles of the rival psychological sects of the last two decades over the status of the image. The functioning of imaginal factors in judgment, in perception, and in thought, (fought over in the experimental studies of the nineties and the first decade of the current century) was, it seems, a pivotal point in the psychology of Aristotle, though admitted only with reservations by Plato and his followers, the Neo-Platonists and Christian mystics. In the sixth century, Olympiodorus anticipated certain recent criticisms of this position, declaring that "imagination, sailing along with reason, makes the waters turbid rather than filling the sails;" thereby antedating in a fashion the findings of the proponents of *Bewusstseinslagen*, 'awareness,' and 'pure thought' processes, who will have it that imaginal thinking is circuitous and childish.

The distinction between sensory and imaginal, between imaginative and memorial (productive and reproductive imagination) was elaborated perhaps as carefully as was possible prior to the development of laboratory technique by Augustine, who noted also the supplementary function of imagery in reading and understanding. Long before his day, the shadowy, filmy character of the visual image, certain peculiarities of after-image, of hypnagogic imagery, as well as the fine gradations between perception and illusion, and the limitations of the functions of phantasy to reinstating or recombining earlier experiences, had been remarked upon. Neither was the investigation of the permutations and commutations through which a given imaginal content may be run the inspiration purely of a twentieth century laboratory genius. This, it appears, was a favorite medieval pastime, if numerous commentaries on the origin of chimaeras, black swans, and New Jerusalems are to be trusted.

There are, of course, many gaps in the available data. The psychologist will miss, in these early records (if we except the brief sketch in *De Anima*) any comprehensive theory of the imaginative function. Of imagination as an integrative factor, operating now as the mere enriching, cementing and coloring of perception and action, now rising to a more dominating rôle in illusion, hallucinations, dreams, poetry and art, culminating in a shadowy but real supremacy in symbolic functioning (supported by neural and organic trends) in thought and understanding, there was naturally no recognition. The generic or abstractive and schematic image of Plato and of Aristotle, listed also by Augustine as in the nature of a 'counter,' was unfortunately lost sight of, to be recovered only after many centuries. Imagery from senses other than the visual while admitted was not stressed, neither was synaesthesis or the matter of imaginal types apparently broached before the modern era. Ideo-motor action and the relation of anticipatory imagery or imaginal rehearsal to imagery were, however, matters of keen concern to the Stoic, as earlier to Aristotle. Epictetus remarks that 'the ass could not walk without images,' and Augustine that 'not even the shortest syllable could be articulated in its order unless imaged.' The possible pernicious effect of 'phantasies' or 'phantasms' upon the morals also received attention, and sample apostrophes were appended for the use of those afflicted, such as 'Phantasie, you are in no manner what you appear to be, etc.' Similar admonitions may soon be expected to appear in our up-to-the-minute text in psychology.

From Plato on, the intimate association of phantasy with the appetitive or emotional nature (of deep significance for later theories of poetry and imagination) was a commonplace in critical thought, remarked upon by Proclus, Synesius and others. Nor did the functional relation of dreams and emotion escape these acute observers, who antedated Freud by nearly fifteen centuries. Many other peculiarities of dreams in dispute today (especially between dreamers and non-dreamers!), such as the occurrences of images within the image, of concomitant thought processes, of the occasional dropping away of the illusion of reality, were observed by Aristotle. As for the vivid phantasying of murders, fights and distant scenes, the cultivation of which is urged by Quintilian upon the youthful orator, it requires no vast straining of the text, no considerable reading between the lines, to discover here the prototype of Jaensch's eidetic imagery.

The possibility of a telepathic function based on imagery was (we learn) broached in the correspondence of Augustine; and the notion of the purely mental character of the image (as over against sensation), a construction fearfully disturbing to the morale of the behaviorist today, was favored by certain early thinkers, though flatly contradicted by the majority. Evidence for and against the localization of imagination in the front ventricle of the cerebrum, thought and memory in the middle and rear, was carefully thrashed out by the early schoolmen; the sub-conscious functioning of the image in recognition was broadly hinted at by Albert, and the exact point in the animal scale at which the imaginal function might reasonably be postulated hotly discussed, the more cautious denying imagery to 'ants, worms and insects.'

Such casual gleanings, however diverting and instructive to the psychologist, are of course incidental merely to Professor Bundy's main purpose, and the above inventory does scant justice to the zeal and acumen with which he pursues his main objective. For it is always with an eye to descrying the first intimations of imagination as Wordsworth conceived it, 'reason in her most exalted mood,' that he scans the early records. The first foreshadowing of the imaginative function as neither 'icastic' (imitative and naturalistic), nor 'phantastic' (illusory and subjective) merely, but constructive or creative as thought itself is creative, and adept withal in conveying thought or emotion, symbol-wise, is the true object of his search.

The vigor and freshness of the author's handling are especially arresting in his diagnosis of the causes of the curious divorce of esthetic theory from imaginative function in the post-classic period. A too literal acceptance by succeeding generations of the diatribe against art placed by Plato in the mouth of Socrates in the *Republic* (and in all probability designed as *reductio ad absurdum* of the premises of Eleatic dogmatism) militated against the truer evaluation of art and of its relation to imaginative functioning, implicit both in the *Phaedrus* and in the philosopher's persistent use of myth and dialogue. To minds in the grip of an uncompromising idealism, the emphasis upon the sensuous origin of the 'icastic' image, and later upon its physiological subtrate in the cerebrum, would inevitably fix upon it the stigma of materialism; while its status as a copy of a copy, in the austere judgment of those for whom the 'immutable idea' alone spells reality, would derogate even further from its value. It is obvious that art products, inspired by imaginal elements defined thus grossly and connoting therefore merely the reëmbodiment of the image in a material medium, would sink to the level of imitations thrice removed from reality. *Phantasia*, moreover, the imaginal viewed as appearance merely, subjective, variable, and fleeting (though later identified with imagination in its freer, more creative vein), could boast even fewer qualifications as representative of the 'immutable idea.' In the distinctions and disqualifications thus conveyed, remarks our author, we encounter the first hint of the divergent ways of fancy and imagination.

Adherents of the more prosaic tenets of Aristotle were equally hampered in the grafting of esthetic theory upon imagination. For in his zeal to cut adrift from the transcendentalism of his master, Aristotle deliberately suppressed, in the *Poetics*, the εἰκασία and φαντασία of Plato, foisting instead the word *mimesis* upon esthetic theory; a stroke whereby he bred confusion in generations of puzzled commentators, condemning art for centuries to quasi-realistic, conventional standards, and a puritanical abhorrence of the 'phantastic,' (a warning to contemporary zealots, who would lay a ban on the use of the term *image* and *imaginal!*). For a brief moment, in the time of Philostratus, under the impact of Egyptian symbolism upon the naturalistic art of Greece, it appeared as if 'phantasia, a better artist than imitation' might come to its own; a hope destined, however, to frustration until the outbreak of Romanticism many centuries later.

Other features of interest to the esthetician in Bundy's narrative are the gradual emergence of imagination as an integrative factor, rather than a mere mechanical power of adding and subtracting items; the recognition of the function of the imaginal as the vehicle of emotion or insight—a dim adumbration of the 'penetrative imagination' of Ruskin, and the emphatic imagination of Groos and Lipps, and also a tentative foreshadowing of some of the artistic tenets of Nietzsche, Croce and Jung. To the psychologist of the visual or aural type, bored with the efforts of crusaders to replace all reference to the imaginal with a semi-scientific lingo of 'muscle-twitches,' we especially recommend these pages; confident like ourselves he will impatiently await the author's resumption of his narrative, and his quest down through the literature, psychological and critical, of the later centuries.

An excellent general index is appended to the volume issued.

University of Illinois ELSIE MURRAY

The American Negro: A Study in Racial Crossing. By MELVILLE J. HERSKOVITS. New York, Alfred A. Knopf, 1928. Pp. xi, 92.

In 1619 the Africans were first brought to America and were sold as slaves. In the three centuries since then the negro in America has come through a long period of slavery and dependence to freedom, of a sort. (There were, of course, some 'free' negroes before the Civil War.) Our knowledge of the ancestry of the negroes in this country is the vaguest, a fact that has, as yet vainly, prompted more than one investigator of late to seek funds for a thorough genealogical research in Africa. Even the processes of aculturation of the negro in American have today received no accurate description. In the present volume Herskovits, a young anthropologist, presents to the general reader the results of his investigations of the physical form of the negro as now found in America. The American negro presents a peculiar genetic problem: the progeny of any intermixture with the white race is called a negro if his African ancestry is known, even though genetically he may be largely of white blood. Moreover, the white blood in the negro seems to be mainly from white males of slavery times, and the principles of selection are not well known. Rankings of negroes in race testing according to degree of African blood are subject to error because of the fact that there are in America different strains of African stock with various pigmentations, and also that in earlier days negroes interbred to a not inconsiderable extent with the American Indians in the Southeastern States. Herskovits does not agree, of course, with the 1920 census figures, that of the 10,463,131 negroes in the United States 84.1% are full-bloods. His method of attacking the problem as to degree of mixed blood was merely that of asking the individuals measured by him to give their descent, a method whose limitations he recognized. He worked among Howard University students, the Harlem population of New York, and a rural community in West Virginia.

Herskovits classified the individuals measured solely on the degree of negro blood as reported by the several persons regarding themselves, with the following results: unmixed, 342; mixed with Indian, 97; more negro than white, 384; more negro than white with Indian, 106; about the same amount of negro and white, 260; the same class but with some Indian mixture, 133; more white than negro, 154; same, with Indian blood, 75; total 1551. Thus only a little more than 20% are unmixed, and between a third and a fourth of the total of 1551 have Indian ancestry. The only checks on the accuracy of these figures are the physical measurements of the individuals of the several groups indicated. It is known that as compared with the European as well as with the American Indian the negro has dark skin, thick lips, broad nostrils, long legs in proportion to total stature (*i.e.* relatively short sitting as compared with standing height), and other such identifying traits. The measurements of these traits were obtained, darkness of skin being determined by matching with mixtures of black, white, red and yellow disks on a color-top and noting the degree of the black segment. A small electric motor that can be kept near the skin would have been better. Results of these several measurements show a striking agreement between reported amount of negro blood and characteristic physical traits, thus establishing, in the investigator's view, the validity of the genealogical report method tried out. The reviewer, however, finds in the book no adequate consideration of the question as to how much these reports of ancestry may themselves have been influenced by the recognizable physical traits measured. The correlation may be wholly or largely influenced by such factors. In such a case, much of the value of the contributions of the research falls to the ground; and the probability of such a weakness at the very base of the work should be removed.

Herskovits then goes on to show from his data that, contrary to the accepted view that great intermixture of races increases the variability, the new amalgam "American Negro" actually shows, except for skin color, less variability in absolute deviation from the mean than the purer strains of most of the various peoples whose intermixture he represents. The important exceptions to this rule are, in nearly all cases, certain West African peoples. But does this then not support the generally accepted view which Herskovits thinks he has put out of court? If the variability of various African peoples is less than that of the American negro, and his is less than that of the Eurpoean, then *the intermixture has increased the variability* of the negro who came to America from those African stocks. Then how can our author hold that the American negro is forming a new racial type of his own? He seems, moreover, to accept the anthropological definition of racial type—small variability—based on the conception, generally verified in biology, that intermixture of types increases variability, and at the same time, as we have seen, to reject it in another relationship. This seems to be a contradiction that needs further consideration and straightening out. If multiple crossings give, in this case of racial mixtures, decreased variability, then smallness of variability surely cannot also in this case be taken uncritically as evidence of pureness of type. One must somehow be consistent. But Herskovits is by no means a careless or superficial student, and he recognizes that his results cannot be final even as applied to the negro. However, he does seem to overlook some of the inconsistencies. The genealogical data gathered by Herskovits indicate a progressive lessening of intermixture on the part of the negro with whites and Indians, which undoubtedly tends toward the stabilizing of the negro "type" in this country.

The chapter on "White Values for Colored Americans" is very instructive to students of race differences, whether psychologists or anthropologists. Herskovits points out that because of the social value of light skin in an environment whose values are so largely set by the white man, there is a

premium among the negroes on lightness of color. He finds evidence of a selective mating in the sense that negro men usually marry women who are lighter skinned than themselves. His data from Howard University, obtained in reply to the question "Who is the lighter, your father or your mother?" showed that in 56.5% of the cases the mother was the lighter, whereas in only 30.3% of them was the father the lighter. He argues that the negro men select light wives for social prestige, while the women select men of ability who "can take care of" them. In this way he would account for the somewhat greater mentality found by certain psychologists in the lighter negroes, a difference which he failed to get from the students in Howard University. According to this view, it is not the white blood soley, if indeed at all, that counts toward making this difference, but the superior ability among the negro men which is selected by the preferred light colored women. The two factors may, of course, work together in the same direction. A predominance of light colored women was also found in the Harlem population studied. To refute the objection that lightness of color may be a sex-linked trait, Herskovits shows that the average degree of darkness of skin as measured in all of the men of the entire Harlem series is 69.5 as compared with 69.8 for the women—a practical identity. What, then, becomes of the lighter men and the darker women? The author does not know, but he offers data which indicate that the lighter men "pass" over into the white group and that the dark women probably become wives in second marriages. In such marriages he notes that the wife is usually darker than the husband, and older.

These views and results, of course, must have more support; and in view of the many uncertainties still existing as to fairness of samplings and as to the basis of the correlation between reports on ancestry and characteristic negro traits, mentioned above, it may well be questioned whether a "new type" of negro is actually being established as contended by Herskovits. Moreover, his data in no way contradict the hypothesis that, in general, white blood has a tendency to increase the intelligence of negroes; nor does his work discount the findings by psychologists as to race differences. The psychologist who has had experience in race testing in America is somewhat surprised at the readiness with which meagerly supported hypotheses are set up in this study. His experience has taught him to expect that such hypotheses will be contradicted in further studies in different localities and by different investigators. This is not mentioned here, however, to discount the work of Herskovits in general, but only to warn the reader against a too ready acceptance of hypotheses as settled facts. The hypothesis is the tool with which scientists work: he may himself demolish it in the next step when he gets more representative data or sharpens his technique.

J. P.

Readings in Educational Psychology. By C. E. Skinner, I. M. Gast, and H. C. Skinner. New York, D. Appleton & Co., 1926. Pp. xxvii, 833.

Volumes of 'Readings' in the various sciences are increasing in popularity. It is difficult to imagine that the thoroughly trained reader will be content to accept such volumes as a substitute for direct reference to the original sources in the literature, although he may find them of value as suggestive of the lines to be followed in reading. If the book to be reviewed here were intended for the latter use, the reviewer's task would be easy. It would be enough simply to state that the authors have collected a large amount of material from a variety of sources, that this material was fairly well arranged and that each chapter was followed by a list of references for further reading. But the authors of this book have designed it for use by the elementary student, basing their work upon the assumptions that "the

students in educational psychology can profitably read much more extensively than is possible because of lack of time or available references," and that "it is impracticable for beginners to rely upon the reference shelf or the library because of limited facilities or the size of beginning classes" (vii).

Sooner or later educators will have to face the question whether a book of this sort, when put into the hands of the untrained reader, may not be far more of a menace than a help. There is ample reason to believe that the average elementary student will receive from this volume the entirely erroneous impression that it provides him with a systematic view of the entire field of educational psychology; or, worse yet, that he will be given the impression that the field of educational psychology is in a state of utter chaos.

The volume is compiled and edited from the standpoint of stimulus-response psychology. In an early chapter, the authors present a *fifteen-line* excerpt from Thurstone's criticism of this type of psychology (in an 'abridged' form!) but, as the rest of this chapter is devoted to most convincing diagrams of stimulus-bond-response relationships, the treatment can be called systematic only with difficulty. Later on, the authors present their editorial comments in stimulus-response terms, announcing, for example, that "learning is primarily the acquisition of new patterns of stimulus-response bonds" (479). But the student is not to be abandoned to any starkly mechanistic behaviorism, for 'purpose' is treated in a most generous manner throughout the book. The reason for this becomes evident when the authors finally capitulate and inform the student, in an editorial comment, that "in a modified behavioristic program . . . it is necessary to include purpose, as well as stimulus and response" (587). It is not difficult to foresee the danger to scientific method that is to be found in a *sixteen-page* contribution quoted from one J. E. Bentley. Here the student is warned that "a continuation of this practise (*i.e.* "materialistic and naturalistic systems of philosophy") would constitute a serious menace to the Christian religion" (744). Two pages further on the student is regaled with the account of a student who testifies that the materialistic methods of psychology drove him right into skepticism! He is told that "fortunately only a few extreme behaviorists have meddled with religion" (752) and is guided along the road to scientific understanding by the author's assurance that "man is not merely a machine, but a person who operates the machinery of the bodily self" (757). In contrast to the sixteen pages allotted to this author, the same chapter provides six pages for Koffka, five for McDougall and one for Morton Prince, while Ogden is allowed five pages for his explanation of the contributions of *Gestalt* to modern education. This arrangement should prove popular with certain state legislatures, but its value for the elementary student is open to question. Without editorial comment, the authors present three articles from the pen of C. L. Redfield, in which the student may learn that "by an increasing amount of mental work performed generation after generation, the mental power of human beings may be raised by successive stages to an indefinite extent" (122). This author states that he has "been able to find but one man of considerable eminence who was born less than 58 years after his paternal grandfather" (123), and further demonstrates the influence upon the child of the age of the father by quoting lists of famous names which demonstrate beyond the shadow of a doubt that the great warriors were sons of men who had not reached their thirty-first year, while the great philosophers came of fathers past their fifty-first birthday. This material is not only included here, but is published without comment!

Of the 833 pages in this volume, 113 are devoted to the subject of intelligence and intelligence testing. Although this is by far the bulkiest chapter of the book, it contains no general criticism of the testing program, and there is nothing in the chapter to suggest that the value of mental tests

is open to any doubt. To reassure anyone who may have been depressed by results of such tests, the authors offer a selection which presents an array of facts about "Great men considered dullards according to school standards" (261) together with a cheery selection entitled "Almost every child good for something" (256). This latter excerpt holds out high promise for every child except "the idiots, imbeciles, morons and morally incompetent children" (256). Aside from the obvious dangers of quotations of this sort, there are other things to confuse the untrained student. Change from editorial comment to direct quotation is indicated only by a small change is type-size, which makes it necessary to refer backward a page or more to discover which type is being used in many of the selections. Few excerpts from the current journals are provided "because of the technical character of much of the periodical literature" (viii), and there is little in the volume to suggest that any experimental work of value has ever been accomplished.

European educators and psychologists are almost entirely ignored. The eight-hundred pages of the volume do not contain a single quotation from Köhler, Bühler, or Claparède. Spearman is allowed one contribution, while Koffka is represented by four distinct quotations. On the other hand, the index contains a full column of references to Thorndike, with James a close second. Hall and Colvin receive generous amounts of space and Dewey provides most of the philosophical background. On the other hand, the emphasis upon social psychology as the basis for education is good and worthy of careful consideration. The chapter on "Mental Hygiene" is a wise inclusion which leads the reviewer to regret that it might not have received some of the space devoted to 'intelligence.' The questions at the ends of the various chapters are rather too difficult for the elementary student; but they should prove to be of considerable value in helping the instructor to formulate clearly his own views on controversial issues.

In the hands of the psychologically trained instructor, the volume may be a genuine aid in the arrangement of facts and as a source of stimulation to more extensive reading. But for the elementary student the book appears to be a source of real danger, a fact to be considered by the teacher who proposes to use it as a substitute for the library shelf and the midnight oil.

University of Illinois JOHN G. JENKINS

Die Kindersprache: Eine psychologische und sprachtheoretische Untersuchung. Fourth edition. By CLARA and WILLIAM STERN. Leipzig, Johann Ambrosius Barth, 1928. Pp. xii, 436.

The importance now attached to verbalized reactions will make inevitable a closer study of the development of language in the child. As a summary of what has been done during the forty-six years since Preyer's pioneer study and as an indentification of certain problems in the field, this monograph in its rewritten form possesses permanent value.

As in the first edition of 1907, about one hundred pages are devoted to the detailed summary of the language used by the authors' two children up to the age of six. The entire range of vocal expression is reported, with the age at which the several elements first appeared and the circumstances that led to their appearance. The second division of the monograph is the most valuable for the general student of childhood, and has been entirely rewritten. In it the writers trace the development of language from the beginning cries of displeasure to the stage at which language is complete in its structural outline. Not only the data from the first part of the study, but all carefully recorded speech histories in the psychological literature are claimed by authors as a basis for their conclusions. Nearly one hundred titles are added to the bibliography of one hundred nine titles in the edition of 1907, and much of the newer work is taken into account in the revision.

Stern's underlying theory is in general accord with critical thought on the nature of development in the child. "Originally the manner in which a person adapts himself to a situation is expressed in a total reaction of the organism in which attitude, facial expression, and action, with the accompanying sound, constitutes an inseparable whole" (p. 123). 'Sound language' (or even 'script language'), which differentiates itself from this organic whole, does not seem to constitute a late stage and it is in reality only an artificial abstraction. Nor is there a separate drive for speech. The tendencies that lead to language are not purely linguistic in nature, but have a much more general character. Vocal expression begins with the cries of pain and distress; sounds denoting pleasure appear before the age of three months, and from these develop the babbling activities. During this period the child reacts to the voices of others and comes to understand the meaning of sounds. True speech begins when the child attaches his own sounds to definite objects and activities. The first words are in reality sentences. There is development in the sentence as such and also a differentiation of the separate words. The first adjectives describe the subjective nature of the child. In personal pronouns the possessive appears before the nominative. The growing language reflects the integrating processes in the child.

The language of children is not a lower grade of a perfect language, but has a form of its own. This form is examined in the concluding part of the monograph. The tonal elements of the early babbling period, the careful analysis of sounds in words, their elision, their transference to different parts of the word, and their doubling, the etomology of the language, and other like topics are discussed in an illuminating manner.

The study will have its largest and most appreciative use among those who trace the development of the child in all of its aspects. Any questions and criticisms from this group will need to take into account the particular point of view which from Stern examines the human personality. After this position is granted, the criticism will direct itself, first of all, to the use of certain concepts that cannot be uncritically accepted to-day as they were two decades ago. Imitation is unduly magnified as an explanation of the growth of language. We need to look for something more basic: the imitating and the ability to do it correctly must be explained. The known laws of learning undoubtedly operate in the acquisition of a language as they do in learning a maze or the multiplication table. Secondly, many of the problems discussed are experimental in nature, and any discussion should be on the basis of adequate objective data. Curiously, Stern is hardly appreciative of elaborate experimental investigations like those of M. E. Smith at the University of Iowa (p. 198) and J. Schlag in the schools of Leipzig (p. 233), although he quotes from a large number of such studies. In spite of the difficulty of the experimental approach, the final answer to a very large proportion of the questions discussed in the monograph—sequence in the development of word knowledge and sentence structure, conditions that lead to the use of certain parts of speech and particular forms of sentences, sex differences in every aspect of language, the effect of learning two languages at the same time, and many others—will not be found until they have been examined under experimental conditions. Various older theories of language development are considered helpfully in Stern's discussion, but such serious work in the field as that of Karl Bühler and that of the behaviorists is not incorporated with the theory presented in the monograph.

It is unfortunate that many studies of the language of children do not refer to Stern and show no internal evidence of having been influenced by his monumental contribution to our knowledge of the subject.

Florida State College for Women P. F. FINNER

Anwendung der graphischen Methode auf Sprache und Gesang. By E. W. SCRIPTURE. Seventy-two Figures. Leipzig, J. A. Barth, 1927. Pp. viii, 114.

It is well-known that Scripture has for many years worked on the problem of devising instruments for making accurate visual records of sound waves, particularly those of the human voice. The first part of this monograph describes his various pieces of apparatus for the analysis of speech and song curves, and discusses the technique of the measurements considered and methods of evaluating observational errors. Careful attention is given to the representations of duration, intensity, quality, and precision of different vowels and consonants produced in various personal and cultural settings. Sentences—declarative, exclamatory, and interrogative—and verses spoken by different persons both in English and in German are analyzed and shown to vary in innumerable aspects, displaying characteristic differences in the degree and the distributions of energy, duration of constant and of changing elements, etc. The energy changes are somewhat periodic, the periods varying with such factors as the content of the speech, the affective state of the speaker and his personality. The means and the percentage variations of these fluctuations are held by the author to be significant of the subject's mental condition. *E.g.* the speech curve of "hippopotamus" as pronounced by an individual with progressive bulbar paralysis shows increasing indefiniteness of demarcation between the elementary characters of the curve—the so-called speech atoms—until the limits of the characters vanish entirely and the elements become indistinguishable. The word begins with marked intensity and high pitch, and rapidly decreases in intensity to a point of considerable weakness, while in pitch it approaches the curve of normal subjects. It is not abnormal in its general aspects. The wave forms are, however, irregular, the duration of elements is increased, and vowels are somewhat chopped, thus decreasing in precision. Normal persons of different nationalities show characteristic differences in pronouncing *pa-pa-pa*. At one extreme the Scandinavians make the *p*s strong, while at the other, the French render them smooth without explosion. Different individuals in any cultural group also have their own characteristic positions between these two extremes, and for each person the position is relatively constant, giving a variability of 5—15%. In progressive general paralysis this may reach 200% or more. Epileptics, on the other hand, show a decreased variability which is claimed to be characteristic and cannot be imitated. The elements of the curve are flattened, making practically a smooth line. Scripture holds, as is well known from his past writings, that he can thus reliably diagnose epilepsy, and he gives several examples to illustrate his success; but he offers no proof of the validity of his diagnosis. As to the underlying neurology of speech, he contends that every nerve lesion accompanied by speech defects conditions a specific speech pathology; that abnormal speech manifestations are constant in cases which are caused by specific nerve lesions; that the subject cannot vary the speech curves of such manifestations to show marked irregularities or temporarily to assume normal aspects. Functional disturbances, on the contrary, show marked irregularities and discontinuities in their pathological aspects. A stutterer, may vary his curves from time to time, even to making them, for short periods, fully normal. Scripture holds that the epileptic, although manifesting decreased variability as mentioned above, may, through personal encouragement, show normal speech curves for short periods of time; and he therefore concludes, in agreement with the view of L. Pierce Clark, that epilepsy has its root in a constitutional disposition and that its seizure is fundamentally but the manifestation of maladjustment. How typical the various "characteristic"

curves shown by the author for different disorders are, can be determined only by extensive experimentation and clinical study. The method is, however, both ingenious and suggestive.

Interesting applications of this graphical method are made both to the instruction of deaf-mutes in oral language and to the teaching of proper muscular control to students in singing. In the former case the individual who already has learned to speak is aided visually in the acquirement of proper emphasis, enunciation, and temporal relations among the different elements, and those who are just learning can study the effects of different vocal efforts. Both may compare their results visually with normal models. In the case of singing the learner may be instructed visually as to the degree of response of different muscles involved in the control of the breath, as well as in various other respects. No mention is made in the monograph of the excellent work in vocal analysis by Miller and by Seashore, though in certain respects the work of these investigators is vastly superior to that of the author.

J. P.

Temperament and Race. By S. D. Porteus and M. E. Babcock. Boston, Richard G. Badger, 1926. Pp. xvi, 364.

The reviewer feels that this book is more valuable for its suggestiveness than for its data. "Race" is used in the popular sense and "temperament" is nowhere defined, even as to the degree of innateness implied by the term; yet the word is used very generously even for traits now regarded as primarily intellectual. Indeed, intelligence tests are included as measurements of temperament. The book is apparently intended for popular appeal; a number of photographs of "typical" boys and girls of the several "races" are included. How typical they really are, seems wholly a matter of personal judgment.

The study reported was made in Hawaii. Following a brief, and somewhat inaccurate, historical sketch of race studies, often very inappropriately selected as to representativeness, the different "races" in Hawaii are severally characterized. Useful data on the history of different peoples in Hawaii are included, but the reader is left to determine the degree of reliability of these data. In this sketch one senses distinct biases on the part of the authors. Twenty-five judges (not described) are asked to estimate the Japanese, the Chinese, the Hawaiians, the Portuguese, the Filipinos, and the Porto Ricans as races according to planning capacity, self-determination, prudence, resolution, self-control, stability of interests, dependability, and conciliatory attitude, or tact. The results agree with the author's characterizations: the Japanese lead in planning capacity, the Chinese in dependability, while the Filipinos and Porto Ricans are low throughout, but relatively nearer the other peoples in tact than in any other trait. One is forced to ask how independently the judges acted of popular prejudices and of the bias of the authors. In "efficiency index" the respective peoples named above were rated as follows: 85.5, 82.6, 51.4, 60.0, 33.0 and 33.3. Between these indices and social adjustment there is found a positive, though not a high, relationship. "Social adjustment is measured in terms of absence of crime, school advancement, occupational classifications, etc. The Chinese have a smaller percentage of murder and manslaughter than the Japanese, and also greater success in school advancement, particularly in the grades. The Japanese are described as alert, aggressive, sagacious in sizing up a situation and foreseeing its trends, selfish and unscrupulous; while the Chinese are patient, conservative, uncomplaining, unselfish, trustworthy in business, and possess little cohesion. Absolute "brain capacity" is measured by an X-ray photography method worked out by Stevenson and Porteus, the validity of which seems

not to have been established; and between such capacity, on the one hand, and intelligence and certain desirable temperamental traits, on the other, a high correlation is found. The number of cases is, however, small and there is little satisfactory evidence that the samplings are good. There is a suspicious enthusiasm for the method. We read that brain growth continues through the whole period of growth in stature even after all the "powers," both mental and physical, have reached maturity; moreover, that whenever arrest or cessation of brain growth has occurred at an early age, idiocy or imbecility exists. This is all to be taken merely on the assertion; no evidence is presented or weighed. Brain size is also in close relation to cultural level in the different "races." The evidence for this statement is culled rather uncritically from many sources. Boys are found to be superior to girls both in brain capacity and in intellectual ability and initiative. There is a blissful disregard of most careful work done by psychologists on sex differences, and even the popular will find numerous dogmatic statements that are hard to accept without question. For tests of intelligence, as well as of temperament, the authors—after a consideration of various tests and data from different "races"—prefer the Porteus maze, which they find eminently suited for race comparisons because of its independence of school training and of language effects, its stress on foresight, etc. This test finds the Japanese superior to Chinese children for all ages above nine years, which, we are told, agrees with expectations based on brain capacity. The book closes with a discussion of racial theories, racial futures, and education in relation to temperament. An annotated bibliography of twenty-six titles, remarkable for importnat omissions, is given.

The entire volume bristles with ideas and bold assertions, and is written in a vigorous (and therefore interesting) style. This is largely due to a marked freedom on the part of the authors from the usual limitations upon scholars, of careful conformity to established facts, and of considerations of data reported by other investigators, which contradict the position taken on the several points discussed. It is precisely for this reason that the book may do much harm to the development of psychology.

J. P.

Criminal Intelligence. By CARL MURCHISON. Worcester, Clark University, 1926. Pp. 291.

This book is the result of perhaps the most extensive mental test survey ever made of the inmates of prisons. Using the group tests, Army Alpha, Murchison examined the prisoners of New Jersey, Ohio, Indiana, and Illinois. The work is almost completely statistical. There is practically no sociological analysis whatsoever, and no qualitative psychological considerations. The book is divided into six parts and thirty-four chapters. Part I contains a preliminary review of earlier work and statement of purpose; Part II deals with white native-born male criminals; Part III with white foreign-born male criminals; Part IV with negro males; Part V with women criminals; and Part VI is a discussion of legal punishment.

Space does not permit a summary of all the findings. Some of the outstanding results on the native white criminal are as follows. There is a correlation between mobility and intelligence of criminals, *i.e.* those who are incarcerated outside their home state are more intelligent than the 'home-grown' variety. There is a segregation of types of crime, those of fraud and deception being committed by more intelligent persons, as measured by the tests, than are crimes of violence, sex and social dereliction. On the whole, recidivists are more intelligent than first offenders, except in the case of fraud and sex crimes. In crimes of force and thievery differ-

ences are not marked. Moreover, the criminal is less literate than the American draft army, but migratory criminals are more literate than the others. Furthermore, literacy does not vary with intelligence in all the types of crimes. Highly educated persons are abundant in some groups and quite lacking in others. The lowest grades of literacy are not found in the recidivists. The author confirms the general opinion that youthful criminals are particularly given to crimes of violence, and that the fraud group is made up of older men.

Interesting comparisons as to industrial status and religious affiliations of the prisoners were made. One interesting fact was uncovered, however, that the criminals from parentage of divergent religious affiliations have somewhat higher rating than do those where both parents are in the same denomination. Some tentative conclusions are given on seasonal variations, length of incarceration, marital concomitants, and height and weight differences.

Some of the facts on the white foreign-born male criminal are these. It appears that the more superior nationality groups, as found in the army testing, produce also the more superior mentality groups among criminals. As to types of crime among the foreign-born group, individuals committing crimes against property tend to rank higher than do those committing crimes against persons. But there is considerable variability here among the various nationality groupings. The foreign-born recidivist is also more intelligent than the first offender. The so-called superior nationality groups of foreign-born produce greater tendency toward recidivism than the so-called inferior 'races.' The analysis of literacy corresponds fairly with that for the native-born group. If literacy is a 'causative' factor at all, "it seems valid to state that literacy correlates positively with the number of criminal convictions, and with the degree of deception and fraud involved" (p. 195).

The investigation of the male negro criminals show that the negro criminal, on the average, is superior to the average negro draft group, but that the entire negro criminal population of the four states examined is inferior to the negro draft from those states. This result is attributal to the inferior intellectual status of the negroes who are confined in states other than their native states, and it contrasts sharply with the situation in the case of the white criminal groups studied. In regard to the facts of intelligence and recidivism, the negroes are much like the white male criminals, but the negro criminals are slightly older, on the average, than the whites. Moreover, on the few cases given, negro Catholic prisoners were superior to the Protestant negro criminals which is the reverse of the situation with the white criminals. So, too, in contrast to the whites, married negro criminals are somewhat less intelligent than the unmarried.

The conclusions about women criminals are very tentative due to limited samples. White and negro women criminals are less intelligent than white or negro male criminals. With white women sex crimes are most common, but this is not true of the forty-one cases of negro women criminals. In the latter group the type of crime is like that of the negro men.

The final section on legal punishment has no relation to anything which precedes it in this monograph and lays the author open to attack as uninformed of work in modern penology. He recommends the abolition of the jury system, of the indeterminate sentence, the release on bond, and the parole system. He also recommends "uniform punishment for the insane, the feeble-minded, and the young" without defining what he means by "uniform punishment." So, too, he recommends that "the third penitentiary conviction to carry an automatic death penalty", and in general "the application of the deterministic philosophy to the behavior of the State as well as to the behavior of the criminal."

University of Wisconsin KIMBALL YOUNG

The Springs of Human Action. By MEHRAN K. THOMSON. New York, D. Appleton & Co., 1927. Pp. xvi, 501.

Thomson says in the Introduction that he wrote this book to satisfy the imperative need for a comprehensive scientific study which would account for the springs of human action. By springs of action he apparently means whatever motivates human conduct. He uses the terms motive and motivation to designate "all the incentives, drives, urges, motives—conscious, semi-conscious, and the so-called unconscious" (p. 20).

The long bibliography and the many footnote references indicate that much literature was examined, but the critical reader will frequently wonder at the selections and at the omissions. Although apparently fond of such phrases as "scientific evidence" the author's sources are usually not those of experiment nor of systematic induction. There is much use of secondary sources when primary sources should have been available. Scant attention is given to the work of the psycho-analysts who have had much to say about motivation. Holt's *Freudian Wish* and Freud's *Beyond the Pleasure Principle* are apparently not known to the author. Hall's *Adolescence* is not mentioned. Coué is given a respectful consideration. Telepathy, which the author seems inclined to accept, is discussed with refernces only to Jastrow and Carrington. For his general psychology the author leans heavily upon Woodworth, McDougall, and Pillsbury. But the authors quoted should not be held responsible for the curious and contradictory statements which appear in this volume. Of such there are many. There are said to be "nine senses" (457). The esthetic sense is aroused by or is derived from a "sense of harmony among the sensations" (458). Ideas have a "tremendous influence upon the psychophysical mechanism" (137). Paramnesia is an hallucinatory phenomenon (297). Rewards and punishments are said to produce after-images. "A positive after-image leads to a repetition of the act and a negative after-image arouses the inhibitory reactions because of unpleasant associations" (130). One of the elements which may factor in a compound is "primary emotions plus sensibilities" (97 f.) Another such element is "emotions plus feelings" (98). Feeling is presented as a complex of sensation and affection, but on the very next page (118) feeling is described as a "general state or attitude" which so far no one has been able to "break up . . . into further elements." The Freudian system is presented as including "two censors, one at the threshold of the fore-conscious and the other at the threshold of the conscious" (292). The author includes a remarkable elaboration of McDougall's well-known attempt to analyze complex emotions (98-104). Fifty-four different compound emotions are itemized and with each there is a list of the chief components. Sentiments and primary emotions and complexes all appear as components of these compound emotions. If the author carries any distinctions in his thinking, between instinct, feeling, emotion, sentiment, and complex, he has failed to make them clear. Mood is presented as a "more permanent affective state of mind" than is emotion "but less so than temperament," and further that "mood is less turbulent than emotion or sentiment" (106). Apparently then mood, emotion, temperament, and sentiment, are comparable states of mind; but upon turning the page one discovers that temperament is something which "determines the type and intensity of the various emotional states" (108). Attitude is defined as a "composite of all the subjective factors of attention, heredity, experience, and present purpose" and this is both "conscious and unconscious" (166). Faculties also appear in the author's scheme. Personality is presented as the agent of an operative will (172), which is described as relatively free—although the limitations placed upon it would seem to leave little room for free action (172 ff.).

There is a "critical faculty" (305) which is asleep during hypnosis. Again, there is a "faculty of the mind not to tolerate a question for long" (325). And so on throughout the 500 pages of the book.

Reflecting upon the experience of reading this book the reviewer recalled that alluring statement in the preface, that the book was to be more than an eclectic presentation, that it was to be a "new presentation." So he went through the book again looking for the new, fearing that perhaps in marking the confused and confusing statements just quoted he had overlooked its contributions; but again he failed to find it. There is, to be sure, an unusual if not unique succession of topics; but the reviewer cannot recall that these topics ever have been considered except as factors in human conduct. It seems after all then to be little if anything more than an eclectic presentation, which might have been of much value but for the author's indifference to logical and psychological distinctions which most psychologists consider important.

University of Oregon EDMUND S. CONKLIN

The Ego and the Id. By SIGMUND FREUD. Translated by Joan Rivière with a translator's note. No. 12 of the International Psychoanalytical Library. Edited by Ernest Jones. London, the Hogarth Press, 1927. Pp. 88.

Although quite a small volume *The Ego and the Id* is full of a great deal of new material and cannot be adequately condensed in any brief abstract. Freud starts out by minimizing the importance of dividing the mental life into the conscious and the unconscious, which division has been one of the primary assumptions of psychoanalysis. He feels that some other formulation must be substituted in order to continue with the researches of psychoanalysis. He, therefore, substitutes the ego, the id, and the super-ego, and proceeds to build up some new conceptions from this.

The id represents the earliest organization of the mental life of the individual and contains within itself the emotional and instinctive forces. It represents the source of energy for the individual. "The ego is that part of the id which has been modified by the direct influence of the external world." All of the ego, however, is not conscious, a part of it being unconscious. The id is unconscious. The ego has the task of bringing the influence of the external world to bear upon the id and its tendencies and endeavors to substitute the reality principle for the pleasure principle which reigns supreme in the id.

The super-ego is formed from the primary identification with the parent. The super-ego is regarded as a "precipitate in the ego" and is a modification of the ego. The super-ego, or ego ideal, as Freud also calls it, derives its characteristics from the way in which the individual deals with the Oedipus complex. The super-ego corresponds in many respects to conscience and from it are derived religion, morality and a social sense.

Freud then goes on to postulate two classes of instincts, the first of which he calls Eros, or the sexual instincts, the second which he calls the death instinct. In a physiological sense, Eros represents the tendency to increase the amount of living organic matter, whereas the death instinct represents the tendency to break down living matter and to increase the amount of inorganic matter.

Freud next brings out several points with regard to mental disease. Certain patients do not improve as they should under psychoanalysis. This is because of an unconscious feeling of guilt which appears to the patient simply as a feeling of being sick. This represents a most difficult type of case to treat since the physician is tempted to use his own personality to aid the patient's recovery. Freud rejects this method of treatment and concludes with what appears to the reviewer to be a most important

statement. "After all, analysis does not set out to abolish the possibility of morbid reactions but to give the patient's ego *freedom* to choose one way or the other."

Freud next discusses the obsessional neuroses and melancholia in connection with the sense of guilt. He feels that in melancholia the ego recognizes its guilt and allows itself to be punished by the super-ego, whereas in the obsessional neuroses the ego has not accepted completely the idea of guilt and attempts to repudiate it by the help of the physician. Freud also states that this unconscious sense of guilt may turn a person into a criminal. He finds that a strong sense of guilt has been present in many criminals prior to the committing of the crime, and concludes that this sense of guilt was not the result of the crime but the cause of it, maintaining that the individual experienced relief at being able to attach the unconscious sense of guilt onto some actual fact.

The id is described as being "totally non-moral." The ego "strives to be moral" and the super-ego "can be hyper-moral." The fear of death is thought to originate in the fear of castration.

The book represents quite a new development in Freud's ideas, a frank admission that some of the older conceptions of psychoanalysis have been incorrect and inadequate and an attempt to introduce new formulations which will be more satisfactory. The book is highly stimulating to thought and can be recommended for very wide reading, whether one accepts or rejects the psychoanalytic doctrines.

The translator is entitled to the greatest praise for giving a translation which is simple, intelligible and of a high literary quality.

Boston Psychopathic Hospital KARL M. BOWMAN

Psychology by Experiment. By L. W. KLINE and FRANCES L. KLINE. New York, Ginn & Co., 1927. Pp. 338.

This is a laboratory manual designed for introductory courses in psychology. After two very brief chapters which serve to orientate the student with respect to experimental method and general procedure in the psychological laboratory, the authors devote the remaining chapters to 109 experiments. The list of topics covered and the relative weight given to each is indicated by the following chapter headings and the number of experiments under each one: movement (including various types of reaction times), 13 experiments; sensations (including the more common psychophysical methods), 29; attitude, attention, and selective action, 11; retention and association, 13; perception, 14; learning, 7; memory and imagination, 11; reasoning, 6; affective reactions, feelings, and emotions, 5.

The number of experiments is obviously and intentionally too great for a single course, but this allows for a selection. Often there is an alternative between an experiment which requires more elaborate or expensive apparatus and one which does not.

Every chapter is introduced by a number of quotations. These are evidently intended to define for the student the concepts which appear in the title of the chapter. It seems to the reviewer that this would be more confusing than helpful to the neophyte, if he can comprehend the rather condensed and isolated statements at all. The author's own explanations of the concepts in a few paragraphs of introduction which follow will probably serve the purpose much better. A topical outline in each chapter covers the subject matter under the title of that chapter. This, in the reviewer's opinion, is entirely out of place, especially at the beginning of the chapter. A logical outline is more or less a personal product and has little significance to another individual unless that other individual has already covered the ground and has followed much the same line of thought. In the student's own mind, an outline is the last thing arrived at, therefore it

might appear in the last part of the chapter if at all. If the outline is included for the sake of instruction in the topics with which the experiments are going to deal, then again, I would point out that each individual instructor would want to present the topics in his own personal way. The book can be used, however, without the inclusion of these outlines. The experiments are of such a nature that they can be introduced in any desired manner.

The experiments themselves have been very carefully worked out, and seem to have stood the test of former use. Some of them are the traditional experiments of Sanford, Titchener, Witmer, and Judd, but the greater proportion of them is new. The student is stimulated with pointed questions and suggestions throughout the instructions so that he may get the maximum returns from his labors. The authors' references for reading at the end of each experiment and the extensive bibliographies at the ends of the chapters should develop scholarly habits of reading the psychological literature as well as habits of genuine research. A list of questions and exercises at the end of the chapter will serve for valuable discussions or for written reports in which the student is required to apply the principles which he has determined experimentally.

In these days, when introductory courses in psychology differ so much from one department to another, and when so many laboratories devise their own experimental procedures and select their own problems, one might think that a laboratory manual would find little place. Yet, probably the majority of departments in which experimental psychology is taught are dependent upon a manual of just this sort. To these laboratories a manual so catholic in its scope should be most welcome. There is a certain value, after all, in placing in the hands of a beginning student a carefully worked out manual such as this one is. The traditional manuals are sadly out of date, not in their methods so much, as in the subject matter which is now emphasized. The reviewer does not hesitate to recommend the manual for use as a guide to students and for its high suggestive value to instructors in beginning psychology.

University of Nebraska J. P. GUILFORD

Heredity and Human Affairs. By E. M. EAST. New York, Charles Scribner's Sons, 1927. Pp. xi, 325.

This book by the author of *Mankind at the Crossroads*, a botanist of wide biological perspective, is of interest to psychologists from two standpoints. In the first place, it includes certain data from ratings and psychological tests as applied both to individual and to racial differences, and it organizes them with other biological data into a genetics of scientific humanism. The psychologist can hardly wish that these data had been somewhat more representative and up to date. The more recent and critical studies are slighted. The psychologist is also interested in the book because it affords him an authoritative and timely statement of the various developments in genetics as they bear on human traits. Convenient summaries of the contributions by various investigators of the heredity of human traits are included. In the interpretations by the author the reader is constantly reminded that the individual's traits are to be understood, in any case, not in the gross terms of his race or even of his parents as complete organisms, but rather in terms of the 'packets of genes' from which he came. These genes are carried in the 48 chromosomes of each parent, which afford an enormous variety of probable combinations in fertilization, and therefore of human characters. Following a presentation of the mechanisms of heredity as now represented in Mendelism, applications are made to the facts of sex determination and of sex-linked traits—hemophilia, red-green color-blindness, atrophy of the optic nerve, night-

blindness (hemeralopia), and coloboma, a defect which usually shows only in the iris, but may affect lens, retina, and choroid coat. The last four defects may prove to be complex types, and coloboma is not yet certainly established as a sex-linked trait. Other serious eye defects that are not sex-linked are also considered, as well as congenital deafness, abnormalities of the teeth, form and color of hair, skin color, baldness, etc.; and, finally, feeble-mindedness and forms of insanity, which may be due to the interaction of very many genes, following, however, the simple and well-known Mendelian laws, are taken up. Effects of marriage between near kin are considered in considerable detail, as well as certain race differences in intelligence and in other traits. While the author recognizes that individual differences within any race are greater than race differences, he attributes practically all great accomplishments by negroes in America to the white blood in them, *i.e.*, to the effects of their genes from the white race. Genetically, race characters are often ill-defined. A valuable discussion is given of the possible influences of nurture on native race differences.

Much emphasis is put on the evidences of degeneration in the human stock of leading nationalities, due to a constantly decreasing reproduction rate of superior as compared with inferior individuals. The author shows convincingly that most schemes for charity result only in the acceleration of this degeneration process, and he looks upon an intelligent birth-control as the greatest hope of solution. "Until birth-control is recognized as a necessary social prophylactic by public health officials and family welfare organizations, this well-intentioned philantrophy and social service is nothing but a brutal gesture to posterity" (p. 307). East sees hope in the working of birth-control methods, even among the lower-ability groups, from the fact that the lower economic strata have shown significant declines in birth rate after the passing of laws in England successively increasing the restrictions on child labor (Pearson), thus no longer permitting children to be made a financial asset.

This book, by a high authority in genetics, is written in an engaging style which should take it into many non-academic circles where its effect is sure to be beneficial toward the establishment of a saner and more scientific view of human life and its possibilities. Definite programs for race improvement will doubtless follow a more general acceptance of the scientific point of view.

J. P.

Psychology: Its Methods and Principles. By F. A. C. PERRIN and D. B. KLEIN. New York, Henry Holt and Co., 1926. Pp. x, 387.

This book is one of the type which grows increasingly popular—a psychology which, while mainly behavioristic, still maintains a respectable family connection with introspection and conscious states.

The section on the "Biological Foundations of Behavior" is largely the physiology of the receptors and effectors, with unusual emphasis upon the glands. Since this is a text in psychology it would be desirable to treat at greater length the mental aspect of the function of these organs. The omission of the facts of color vision and auditory experience is undesirable although there is some compensation in the inclusion of more than customary material on pain, the static sense, and kinaesthesis.

The chapter on the "Psychological Foundations of Behavior" is the best chapter in the book despite certain questionable or inaccurate statements. On page 96 there is a confusion in the statement of the three possibilities of the sequence and duration of stimulation in the conditioned response; (1) and (3) as there stated are the same. On page 90 it is suggested that lightning as a cause of fear is a substitute stimulus for the idea of

destruction and death. Would it not be better to point out that though this is the common explanation of the layman the fear reaction probably came first, and for most people still does, come to the *thunder* and that the suggestion of destruction and death as the cause of the fear is a rationalization? Again, the term *selection* is said to be preferable to *attention* (p. 79) but the latter word is the one which the authors continue to use (pp. 292, 364, etc.). The topic of conditioned emotional reactions, which is merely suggested in most introductory texts, is very well presented.

Two groups of factors in human motivation, the physiological and the social, are recognized. The treatment of the physiological motives (which includes emotions) is satisfactory; but that of the social motives is inferior because it is largely descriptive rather than explanatory. That there is a mutual relation between biological and social motives is stressed; but the nature of this relation is not sufficiently demonstrated. Since the mechanism of the conditioned response has been used so ably in other parts of the book (in the elimination of instincts and in the explanation of learning, for example) the reader wishes that it had been pressed into service again to make the origin of the prestige motive" less obscure.

There are two especially good features in the chapter on learning: the discussion of the part which consciousness plays in the learning process, which is an original contribution, and an unusually clear summary of a number of theories of learning.

Much of the last chapter is devoted to the proof of the theme that thinking is a form of substitute behavior. The mechanism of the conditioned response is again successfully utilized to account for the process of substitution of certain symbols for overt behavior. One wishes, however, that this excellent account might have been somewhat condensed and more attention given to various types of mental activity such as reasoning, memory and imagination. The interest of elementary students in these topics is strong and should not be neglected. These subjects could and should be developed without a reversion to faculty psychology.

University of Chicago DORRITT STUMBERG

A New Method of Mental Testing. By J. J. STRASHEIM. Baltimore, Warwick and York, 1926. Pp. 156.

Strasheim believes that mental testing has reached an impasse for two reasons: (1) there is no acceptable definition of intelligence; and (2) there is a need of new research upon intelligence in order to free the present tests from their limitations and to give them new life.

He starts out with the theory of Spearman that intelligence is a purely cognitive capacity involving the ability to perceive fundaments and to educe relations and correlations between them. He devised tests especially to measure this capacity.

The first of three groups of tests consists of three 'situation' tests. They are similar to the Binet tests of comprehension in which the child is asked "What would you do if—?" In Strasheim's tests, however, the situation is introduced in the form of a story. The tester is told in the beginning how to meet a certain kind of situation. The story is told a number of times with variations. Every time the testee has to meet similar but increasingly greater difficulties. He may use his previous knowledge gained in the test, or perhaps also from his earlier experiences, but must modify his method each time to fit the situation.

The tests seem well adapted to demonstrate Spearman's theory of intelligence but seem to be highly impractical as a psychometric tool. Too much will depend upon the individual tester. The procedure is long and tedious and the results are hard to quantify.

A second group of tests was devised to measure intelligence as it plays a rôle in learning. The testee is given a series of 'pencil and paper' mazes in which he is to discover by repeated trials the general principle which determines the correct path. These tests seem to be more suitable for quantitative purposes but the author has done little to validate them or to establish a scale. The number of testees was small and they were merely classified as young or old, bright or dull.

A third group was composed of cancellation tests. The testee has to cross out from 2 to 8 different letters wherever they appear in pied type. The letters to be crossed out are kept in mind either perceptually or by memory. Another variation is to have certain letters crossed out only when they appear in combination with other letters.

The author makes detailed analyses of the results from these tests, demonstrating their relation to Spearman's theory. He also points out at every opportunity the application of this theory to the problems of education. There are occasional polemics against the theories of learning which make of it a purely mechanical 'chance' process, *e.g.* the trial-and-error notion. What he has to say on this subject will bring relief to those who have a chronic autonomic tension set up by mechanistic theories of learning.

The book will be a disappointment to those who expect to find, as the title implies, 'a new method of testing,' all ready for use in measuring minds. To others it will be highly suggestive, if carefully studied, in its approach to the problem from a particular point of view. In the reviewer's opinion, however, it is the theory which is new rather than the tests. The positive gain is the attempt to see clearly what the usual tests of intelligence measure. Perhaps this attempt to develop tests carefully worked out on the basis of an *educed* theory is a typically British method as opposed to the trial-and-error methods of the hyper-kinetic American psychologists who are striving to accomplish the same end.

Kansas University J. P. GUILFORD

Statistical Methods for Students in Education. By KARL J. HOLZINGER. Boston, Ginn & Co., 1928. Pp. viii, 372.

This contribution is too thorough and discriminating to be "just another statistical text." In general level of difficulty it is harder than a number of texts which in their endeavor to be elementary gloss over difficulties and present statistical issues in the atmosphere of great simplicity which do not belong in this atmosphere. This may be illustrated by a quotation from page 27; "It will be noted that the median is here defined as the middle score." It would be characteristic of spuriously simple texts to stop at this point, but Holzinger goes on, "For an odd number of cases this definition offers no difficulty, but with an even number the use of the value half way between the two middle scores is a convention supplementary to the definition." Any number of similar illustrations wherein difficulties are made explicit rather than suppressed could be given, and in each instance the procedure followed makes for a sound treatment. Certain difficulties, such for example as the numerous situations in which small populations alter statistical measures, are not uniformly treated, but the book goes so much further than the customary statistical text in matters of this sort as to give a most favorable impression.

It is obvious that the author knows the mathematical shortcomings of students of education taking statistics, for he treats of certain matters ordinarily assumed, such for example as an elementary knowledge of logarithms. In the treatment of foundation mathematics the book is rather lacking in uniformity for a quite adequate elementary treatment of logarithms is provided but a very inadequate treatment of determinants.

Logarithms are treated in Chapter 4 and determinants are taken up in chapter 15, so those who have meanwhile dropped by the wayside never realize the later shortcoming.

The writer finds nothing particularly new in the subject matter and this is commendable in a book of this level, for it is much easier and of less value to devise novel techniques than to use soundly well-established formulas.

The later chapters are enjoyable reading for one with some slight mathematical foundation and should easily induct the high school teacher of mathematics into some of the mysteries of statistics.

Error in a book of this extent is practically unavoidable. The writer has noted just one that to him seems to be serious. On page 235 is given a formula for the probable error of the difference between two uncorrelated means. This is followed by an illustration wherein as a preliminary step "two groups of pupils were equated with respect to intelligence and initial ability." Thus the illustration is a problem wherein there is correlation between means. The significance of a difference is the single universal problem of statistics and it is unfortunate that there should be an error upon this fundamental matter. The later treatment of differences, pages 242-243, is correct, but here the difference involved is that between correlation coefficients and not a difference of one of the more common types—between means, individual scores, or between proportions.

On the whole the book is well written, of excellent make-up, and sound and scholarly.

Stanford University TRUMAN L. KELLEY

Mental Abnormality and Deficiency. By S. L. PRESSEY and L. C. PRESSEY. New York, The Macmillan Co., 1926. Pp. xii, 356.

The authors state in the Preface that they have written this book with two purposes in mind: (1) To formulate material for a college course; and (2) To furnish a source of practical value to those interested in various special fields in which problems of personality and adjustment are frequently met. They repeatedly emphasize that their work is for students, school officials, social workers, and personnel managers.

The style and general plan of the book are very acceptable. Special note should be made of the good bibliography and the glossary of technical terms. There are certain high spots in the book, such as the discussion of feeblemindedness, the epileptic personality, and the degree of mental abnormality that the supposed normal shows. The case study outline that is presented seems rather awkward. It seems more practical to be less rigid in temporal divisions than this provides.

The reviewer takes issue with the fundamental note of the book. It is a book written by psychologists for lay people; so far as clinical psychiatry is concerned, therefore, it seems not only useless but pernicious to encourage these readers to train themselves to proficiency in the differentiated types of psychoses. In justice to the authors one must add that they frequently advise consultation with psychiatrists, but it does not seem that this counsel acts as an effective check. The book suffers as do most small texts, from a certain amount of didacticism. There are many isolated statements which a medical specialist would probably question, *e.g.* "cases of extreme excitability are comparatively common, and are, very likely related to disturbances in the functioning of some one or more of the ductless glands" (p. 4); 'a complete physical examination by a physician is usually desirable whenever this is possible. But since an examination is often impossible—frequently it is not called for" (p. 36); "type too fine to be easily read by the patient (manic) when in a normal condition is now read without the

slightest difficulty" (p. 49); "she had a complete collapse and died in a few days of apparently a broken heart" (p. 104); the listing of an irregular pulse as a common symptom in neurasthenia (p. 110); the statement that one who is merely mentally peculiar and not psychotic can consider himself God Almighty, Himself (p. 136); that an investigation of the personality of the dementia praecox, "very likely shows that the patient was one of those children who develop slowly. Walking and talking were delayed and throughout childhood the patient was somewhat sickly" (p. 150); "such individuals (manics) may turn up with fair regularity every few years with a touch of excitement, amuse and delight the hospital for a while, perhaps, then go through a depressed period and then return to the world again" (p. 167); "the family of a person suffering from chronic alcoholism is likely to be alcoholic and defective" (p. 187); "the person who develops a delirium on one or two degrees of temperature is probably psychopathic" (p. 209); "congenital syphilitics are almost invariably feebleminded" (p. 221); and that "national prohibition has accomplished great things in regard to the alcoholic psychoses." (p. 296).

Although many of these statements might be tenable as unusual personal opinions of the authors, a text-book that appeals to a group without adequate scientific training for sound critical judgment in this field, is hardly the place for them. It seems to the reviewer that a book on a broader biological and economic basis would serve its purpose better than one with clinical discussions and clinical case reports as the dominant note.

Boston Psychopathic Hospital MANFRED S. GUTTMACHER

Ueber den Aufbau der Wahrnehmungswelt. By E. R. JAENSCH and associates. Leipzig, J. A. Barth, 1927. Pp. xxx, 567.

Here are bound together within the compass of a single volume fourteen papers which have been previously published in psychological journals. The content is fascinating, often tantalizing, but the volume is exasperating. It lacks the continuity which a well planned volume would have, and in bringing together a lot of papers there is produced inevitably much repetition. There is in the book much that one desires to refer to again and again, but there is no index. For a volume of more than five hundred pages this is a serious omission. There is, to be sure, a very full table of contents which is helpful although not entirely consoling. Nine of the papers are by Jaensch alone, three others by Jaensch in collaboration with Reich and Freiling, one is by Freiling alone, and the other is by Kröncke. There is no summary of the findings.

Much of the material presented is now well known to students of the eidetic image problem. The first paper presents in some detail a number of interesting and sometimes clever methods for the study of and detection of the eidetic image. It includes also a description of the T and B types. In other papers the play of the eidetic image in the visual perception of location, the effects of attention, and the effects of eye-movement are studied. There are some short papers which compare the experimental findings with a small number of anthropological works, with the conclusion that the eidetic image is common to primitive peoples. Some very alluring experiments are presented on what is termed the *Kovariantenphänomenen.* When some threads are so placed that there is no clue to their relative distances away except the appearance of the threads themselves, if one is moved another appears to have been moved also. This is likewise examined for its relation to the eidetic image. There is a long paper on the place of the eidetic image in the development of perception and a very long one (130 pages) on the biological relations. This last is primarily a detailed presentation of the development of the eye, emphasizing especially its close relationship to the brain.

The argument for an eidetic stage in development out of which have been differentiated our perceptual and conceptual processes, and for the continued influence of eidetic images in many individuals, is attractive and as the literature accumulates is almost convincing; but it can never be wholly so until there is experimentally obtained evidence more reliably based than most of that to be found in this volume. Frequently the experimental researches reported here were made on not more than half a dozen subjects, rarely more than fifteen. Differences are presented without any consideration for their reliability. In fact there is an almost complete indifference to the technique of statistical method. Perhaps the indications of these studies can be supported by research with larger numbers but until that is done one must hold tenaciously to the position of suspended judgment.

University of Oregon EDMUND S. CONKLIN

Brains of Rats and Men. By C. JUDSON HERRICK. Chicago, University of Chicago Press, 1926. Pp. 365.

Professor Herrick's book is a reëxamination of cortical function and its development in mammals, in view of the work of Lashley on the learning processes in rats.

Eight chapters are devoted to the evolution of the various parts of the brain, their functions, and interrelations. Three chapters are given exclusively to the experiments of Lashley with the author's own interpretation of the results. He agrees with Lashley that the cerebral cortex of the rat participates in habit-formation, but does not regard all parts of the cortex physiologically equipotential. There is some localization of function, at least in the projection centers, according to the author, but there is much more definite localization of function in the brain of man. Whereas the cerebral cortex of the rat exerts a relatively greater tonic effect over the lower centers, the human cerebrum has a relatively greater phasic function.

In the remaining five chapters the author deals with more speculative topics, such as the 'subconscious,' 'consciousness vital function,' 'vital reserves,' and the 'ends of human conduct.' He belabors himself over the old problem of mind-body relationship, and insists again and again upon the existence of consciousness, or 'introspective' data. While committing himself to a deterministic monism (17 ff.) as the essential position of a scientist, he later comes very close to an interactionism when he insists that awareness is a link in causal chain of events (Chap. XVI). While one may be convinced from his discussion that consciousness is an observable scientific datum, one does not feel satisfied that the author has found a logical place for it in a monistic system.

The interpretation of the important work of Lashley by one so competent as Professor Herrick is enough to attract the attention of anyone interested in the problem of learning or in cortical function in general. Although the book gets its motive and its title from this source, relatively little space is given to it. A great service is rendered, however, in the collection and interpretation of other recent anatomical and physiological data coming from various sources, and its application to the problem of the evolution of brains.

The material, on the whole, is presented with unusual simplicity and clarity, in view of its technical nature, so that the intelligent layman may find the book not beyond his depth, and interesting as well as instructive.

University of Nebraska J. P. GUILFORD

The Natural History of our Conduct. By W. E. RITTER and E. W. BAILEY. New York, Harcourt, Brace & Co., 1927. Pp. vi, 339.

This is a delightful study in comparative psychobiology written with a real charm. It deals primarily with the development of psychical activities as adaptive agencies. The author draws upon a wealth of material acquired through a mellow life of active work in zoölogy performed on broad biological and philosophical lines. After having passed three score years and ten Professor Ritter confesses that he has been "unwilling to suppress the emotional aspect of his responses to nature to any such extent as many scientists seem to do." This forms a dominant motif in his work and contributes largely to its charm. Neither as psychologist nor philosopher in the specialized sense of these terms does he make any claims for himself.

He assumes that brute activity lends as much assistance to the understanding of human activity as does brute structure to the understanding of human structure. Professor Ritter draws upon data from the lives of the protozoa as well as from the lives of the anthropoids. He recurrs to the similarities and differences in human activity. One cannot claim that in this work there is anything in the approach or in its material that is startlingly new. It contains, however, a wealth of interesting data well correlated for its purpose. There are many interesting discussions, among these are: the worth and limitations of paleontological evidence; maladjustive activity in its four phases resulting in waste of time and energy in useful materials, in injury to kind and in self-injury; and finally the tenet that man's superiority is not due to his cerebral cortical development alone but to the anatomy and physiological activity of his hands as well.

One misses certain pertinent discussions to which the reviewer feels Professor Ritter could add evidence of value. For example: Watson's work with infants and the questions of the inheritance of acquired characteristics, particularly in view of the recent important contributions to this subject.

There is a good deal of unnecessary repetition that could be omitted in subsequent editions, *e.g.* on page 162 there is a paragraph repeated verbatim from page 126. The author states and restates too frequently the aims and purposes of the volume.

On page 59 the results ascribed to the cutting, in the cat, of the vagus nerve are actually those that result from its stimulation.

Professor Ritter states in his preface that he is responsible for the material in their work and that Dr. Bailey, his collaborator, is largely responsible for its organization, readableness and cogent presentation. It must be said that she also did her work extremely well.

Boston Psychopathic Hospital MANFRED S. GUTTMACHER

General Introduction to Psychology: A Survey of the Programs of Psychology. By C. R. GRIFFITH. Revised Edition. New York, The Macmillan Co., 1928. Pp. xix, 607.

The second edition of Dr. Griffith's *General Introduction to Psychology* has been largely rewritten and amplified. The volume aims to present between the covers of a single book a comprehensive view of modern psychology. The treatise as it now appears is divided into the following parts: I. The foundations of psychology; II. Genetic psychology; III. Social psychology; IV. The psychology of the unusual and of the abnormal; and V. Psychotechnology.

In the first section, the usual topics of general psychology are treated in outline fashion. The biological point of view is maintained. A short history of psychology and a consideration of the great schools and of explanatory principles in psychology is also given in this part of the book.

The evolution of mental life is the main topic considered in the second part of the book, which is entitled "Genetic Psychology." The development of the animal and the human intelligence is considered. The inheritance of intellectual traits is clearly treated and the point is well made that the organism must always be thought of as the *product* of heredity and environment, and not as the *sum* of these factors.

In the first section on social psychology, general problems are first examined and then the "monuments to mental life," such as language, the system of law, and religion are considered.

A treatment of individual differences is aptly used as an introduction to the pages devoted to a consideration of abnormal psychology. Intelligence, mental diseases, hallucinations, dreams, occultism, hypnotism, and many other topics are here considered. It is rather surprising to the reviewer to find the ordinary sensory illusion treated in this section of the book.

After a consideration of the relationship between pure and applied science, the author turns in the section on "Psychotechnology" to consider character analysis, educational psychology, and the various aspects of psychology related to legal procedure, medicine, and commerce.

The book is certainly a valuable one. The student who masters it can no longer be ignorant of the secret that psychology as it stands today is a true miscellany. As one reads the informative pages of this book, he discovers that the psychologist is indeed versatile. He is experimentalist, or as the author prefers to say, "experimenter" in his own right. He is system-maker and systematizer of systems. He is neurologist and endocrinologist. He is a philosopher concerned with the freedom of the will and the existence of Deity. Last, but by no means least, he is advisor to that important American academic figure, the football coach.

Brown University LEONARD CARMICHAEL

The Expansion of Rural Life: The Social Psychology of Rural Development. By J. M. WILLIAMS. New York, Alfred A. Knopf, 1926. Pp. xvi, 346.

This is the second volume of a series on the development of what the author regards as a typical rural community in America. The first volume is *Our Rural Heritage*, published in 1924, which covers the early developments. Several other books have come from the author's pen, dealing with the sociological aspects of the same community. Central New York was chosen as the location for study because (1) it is the birthplace of the author, who has studied the community directly for a period of some twenty-five years, (2) it was settled by a variety of nationalities—English, Dutch, Huguenots, Germans, Scotch, Irish, Welsh—and therefore affords opportunities for observations on the mixing of different cultures, and (3) pioneers went from this section to build up various communities further west. The identity of the community studied is concealed; consequently the reader has no adequate check on the reliability of the sources used by the author. Fortunately, from this standpoint, the work does not pretend to be scientific, and no one would make such a charge. There are no references whatever to standard psychological investigations. The present volume traces the development of the community down to the present time. The two volumes in this series, on what the author calls "Social Psychology of Rural Development," record the attitudes of the people toward various of the factors affecting their lives, and the changes that are wrought in these attitudes through the successive developments that have taken place due to inventions, improvement in transportation and communication, scientific progress, etc. A long review is not justified here, but it is relevent to say that the two volumes are very informative as to the

general life of the community. The evolution beliefs, attitudes and methods of coöperation, and the differentiation of interests are clearly portrayed. How much of this is really typical and objective fact, independent of the selection and bias of the writer, is the problem that is unfortunately left to the reader to solve. The treatment is, however, both interesting and suggestive to the social psychologist; because the data reflect popular views in their natural development, and give the roots, however reliably or unreliably, of many current beliefs and practices in rural communities of this country. It would be valuable to have thoroughly scientific studies of typical communities, but such work can be done only by the coöperation of scientists in many fields.

J. P.

Applied Psychology. By A. T. POFFENBERGER. New York, D. Appleton & Co., 1927. Pp. xx, 586.

Evidence of the practical tendency of the day and the consequent rapid expansion of applied psychology is furnished by the mere bulk of this edition, which succeeds "Applied Psychology" by H. L. Hollingworth and A. T. Poffenberger. The earlier volume, published in 1917, was little more than half as large. Numerous experiments performed since that date are described by Poffenberger, and other features are added to the text, which in the main follows the outline of the former book. The new footnote references are of particular value. The volume, comprehensive as it is, could not of itself survey the expensive field. It does, however, indicate fairly exhaustively the many lines of application which psychology has found.

The book is divided into two parts: (1) psychology applied to individual competence; and (2) psychology applied to occupational activities. Applied Psychology is defined as that "applied to the affairs of daily life." Its fundamental fact is asserted to be "that the individual is the unit of action." There follows a study of the individual and factors influencing him—heredity, age, sex, environment, and drugs. Special chapters are devoted to learning, thinking, suggestion and rest. Part II discusses the principles of vocational psychology, the rôle of judgment and methods of measurement, then proceeds to specific results from experiments in industrial psychology, business, law, medicine and education.

The point of view is dynamic. Among the general topics taken up are reflexes, instincts, racial differences, genius, mental defects, skill, practice, mental growth, the overlapping of traits attributed to sex differences, and phenomena of work and fatigue. The environmental conditions of ventilation, illumination, and distraction are stressed. The difficulty of obtaining control subjects in experiments on the effect of drugs, it is claimed, makes the validity of results doubtful.

An account of the search for objective measurements, with its rewards up to date, occupies much of Part II. Vocational selection and guidance, general and special capacity, character and interest, require specific tests. The maximum return for labor is of prime importance to industry, and the consumer's satisfaction is of great significance to business, while economy of effort and the increased efficiency of the human machine are common problems which must be solved by adjustment of conditions. Here we encounter difficulty in removing traditions and superstitions.

The final stage sought by the law is prevention of crime. To this end, the causes of crime, determination of guilt, limitations of testimony, and treatment of the offender are studied. Again, in medicine prevention is coming into prominence. Before diagnosis of abnormal behavior can be made, there should, the author contends, be a study of the normal. In treatment of disease as well, psychological factors are present. In this connection he suggests a course in experimental psychology, to be introduced

into the curriculum of the medical schools. Only the main directions of application in education are indicated, as other publications cover this field.

To those who wish a text which shall form a satisfactory foundation for a survey of applied psychology, and to laymen interested in the outlook of application, not by 'quacks' but by sound investigators, the reviewer recommends this book. You will find the material well compiled and conveniently arranged, and the style lucid.

Cornell University — FLORENCE WOOLSEY HAZZARD

The Effects of Music: A Series of Essays. Edited by MAX SCHOEN. New York, Harcourt, Brace & Co., 1927. Pp. ix, 273.

This volume consists of a series of twelve experimental studies of the effects of music. Eight of these studies are chosen from the papers submitted in a competition conducted by the American Psychological Association in 1921 for the most meritorious research on the subject. The remaining four papers are reports of investigations carried on in the psychological laboratory of the Carnegie Institute of Technology by Dr. Max Schoen and Dr. Esther L. Gatewood.

The prize-winning study, contributed by Professor M. F. Washburn and her assistants [see "A Correction" this JOURNAL, p. 675], deals with the effect of immediate repetition on the pleasantness or unpleasantness of various types of phonograph music. They find that repetition tends to decrease the pleasantness of popular music but to increase the pleasantness of seriously classical music. Among the problems which are dealt with in the other studies are types of musical listeners, the nature of musical enjoyment and its relation to specific emotional effects, the organic effects of music (as recorded objectively), and the mood effects of music.

The volume as a whole represents a significant contribution to the psychology of musical appreciation, but perhaps its greatest significance is methodological. The authors have shown by their results that such 'complex' experiences as the enjoyment of music may be successfully studied by the introspective method. The reports, for the most part, are reports of the effects of musical selections in their entirety, as played on the phonograph, rather than of the melodic fragments such as psychologists have usually worked with in the past. To be sure, none of the results are final. Each study raises more problems than it solves. But that in itself is no condemnation of the work done. Students who are interested in this field of psychology should find the book a valuable source of orientation for future investigations.

Cornell University — E. FRANCES WELLS

A Study in Moral Theory. By JOHN LAIRD. New York, Macmillan Co., 1926. Pp. xviii, 322.

This volume is a subtly reasoned and well polished essay. Its general point of view may be expressed by the following quotation: "anyone who takes account of his action should also take account of his principles" (p. 9).

The preface notes that the argument may be divided into four parts: the first, analytical; the second, psychological; the third, sociological; and the last, philosophical.

The distinction which characterises moral enquiry is between reasons that are simply explanatory and reasons that justify or condemn. Moral theory deals not with the antecedent facts that have brought things about in a certain way, but with the problem ought they to be of this kind. Plainly the only consideration relevant to the justification or con-

demnation of an action is its value or lack of value—its goodness or badness. Moral theories usually deal either with the conception of value, or the conception of obligation. "Of these the former, by itself, seems insufficient; the latter, when justified by the former, to be the proper object of moral study" (p. 196), for although right conduct must be justified by its value, it is conduct with which we have to deal. The mere existence of good or beautiful things is no moral matter. Imperatives thus define a most essential field of moral study, and their justification and illustration is extended to some length.

Since "moral theory has to do with justifying, not with explanatory reasons" (p. 119), and since psychology is an explanatory science, it follows that psychology can never be the basis of ethics. But such notions as "moral obligation" are mere words unless they actually apply within a certain psychological field. The "admission of obligation on our own part seems to presuppose, first, that the authority of conscience depends upon the possession of a certain species of knowledge" (good and evil); and "secondly, that these beliefs concerning good and evil (while not infallible) are not inadequate to the requirements of our action in a given situation, in so far as moral consideration really are binding; and thirdly, that these beliefs must be capable of effective influence upon our conduct" (p. 201). The truth of these assumptions are vigorously defended.

The next chapter deals with collective action. The final chapter places ethics in a natualistic metaphysics; but insists that biological principles do not determine ethical ones, and that, "if our convictions are not shams, all our directions in terms of values must have an absolute, not a relative, basis" (p. 303).

University of Washington RALPH GUNDLACH

Vocabulaire technique et critique de la philosophie. By ANDRÉ LALANDE. Paris, Félix Alcan, 1926. Vol. I, 1926, pp. vii, 499; Vol. II, 1926, pp. 563.

This work has been in the making for many years, the general idea of it and the method to be followed in its construction having first been sketched by Lalande in an article published in 1898, and proposed to the International Congress of Philosophy in 1900. It is one of the publications of the Société Française de Philosophie, and has been published in parts and reviewed and criticized by the members and corresponding members of this society. Views and discussions by different members are given in footnotes with names attached. The terms defined and discussed in the text are followed by their foreign equivalents or approximate meanings (Greek, Latin, English, German, Italian). The terms considered are not only terms peculiar to logic, moral philosophy, esthetics, and metaphysics, but also terms used in psychology, sociology, and various other subjects related to the four members of the philosophical group named. In all cases, however, the treatment is from the philosophical point of view. As a consequence of this fact terms used in modern objective psychology are, of course, largely neglected. A supplement of 88 pages, added mainly by Delande, and therefore not carrying an indication of authorship of different items, gives additional terms and also supplements to the treatments of terms occurring in the main vocabulary. For the convenience of the reader the terms discussed elsewhere in these volumes are marked with an asterisk. In this supplement one finds, as new additions, such terms of interest to psychologists as aptitude, attitude, behaviorism—which, however, gets less than four lines—correlation, language, structure, variability, etc. Under "form" and "structure" the psychology of *Gestalt* gets short notice.

These two volumes are especially useful to psychologists working on the historical aspects of psychology.

J. P.

Measurement of Intelligence by Drawings. By FLORENCE L. GOODENOUGH. Yonkers, N. Y., World Book Co., 1926. Pp. 177.

This is a description of an experiment to ascertain the extent to which the nature of the drawings made by young children is determined by their intellectual endowment. Other investigations have suggested that concept development is a greater desideratum in effective drawing than specific esthetic or manual aptitude. It cannot be asserted as proved in the face of such contradictory evidence as that of Leta Hollingworth showing the paradoxical contrast in merit between drawings of certain children having high and low intelligence quotients.

Goodenough has, however, brought to light data from several thousand children for objectively differentiating between those cases of drawing ability due to level of intelligence and those due to other factors.

The child is given the task of drawing a picture of a man without model, copy, or suggestions. Test results are influenced by practice in drawing the human figure, but are relatively unaffected by the ordinary formal drawing instruction of the primary school grades.

Because of the scoring method, artistic qualities per se of the drawing become negligible factors and subjective judgment is eliminated. A few points showing increase in grades of difficulty on which the drawings are scored may serve to suggest the character of the complete scale of fifty-one units: head present, legs present, length of trunk greater than breadth, nose, mouth, arms and legs, shown in two dimensions, correct number of fingers, head in proportion, eye details, etc.

The validity and reliability of the test is approximately that of the other more commonly used group verbal primary level tests and higher than most non-verbal scales. Its particular value lies in its serviceability for testing mentality of deaf children and foreign children having language difficulties. The other major advantage is its brevity, requiring no more than ten minutes for group administration and two minutes for individual scoring. The probable error of estimate of an I.Q. is approximately 5.4 points at all ages from five to ten years.

The author's chapter on the psychological interpretation of drawings is consistent with and might even be regarded as excellent support of Piaget's thesis of concept quality inherent in the nature of the young child's mind.

Vassar College MIRIAM C. GOULD

La representation du monde chez l'enfant. By JEAN PIAGET. Paris, Félix Alcan, 1926. Pp. 424.

This is the third of a group of studies by Piaget dealing with the qualitative aspects of child thinking. Piaget is not concerned with measurement of the child's mentality, nor with charting growth, nor with eliminating errors. He is a zoölogist as well as a psychologist and approaches his material with the bent for minute classification and for discovery of structural rather than functional status of the child's mind. His evidence includes soliloquies, conversations between children, their mutual questions and explanations of cause, of motivation, and of justification.

The problems of children's ideas of reality and causality are treated as confusions: initial confusions of object with symbol or name, of percept with object, of percept with image, of attributes with things, of dreams with events, of being with participation. Added to these are the accidentally acquired confusions in the child's concepts of relationships between them.

A section on child animism discusses the immature concept of life as disclosed in the tendency to attribute consciousness to things. Factors

predisposing to this mechanism and conditioning it are the process of systematization of ideas, the process of extension of vocabulary and of awareness of the universe.

The last section deals with children's early concepts of the origin of stars, water, trees, earth and the significance of this character of primitive thinking. Piaget does not offer any markedly original interpretation, nor does he ally himself with mystical or ulterior modes of analysis of these notions common to all children. From the author's point of view, the child world is not the fiction of unpredictable imagination, but a series of stages of explanation logically reached by selecting and assimilating new materials with old in accordance with laws inherent in the developing thought process, proceding from dynamic substantialism to magic and from magic to static explanations. Quite independently of adult help or supervision the child assembles step by step the basis for the construction of his concepts appropriate to the stage of his mental structural development. The adult thought elicited by children's questions and demands, plays far less of a rôle in actually molding a child's ideas or determining their quality than has been the prevalent supposition. The adult's words cannot have for the child the adult meaning and it has long since become impossible for the adult to think in the manner of the child.

Vassar College MIRIAM C. GOULD

Economics and Human Behavior. By P. S. FLORENCE. New York, W. W. Norton & Co. Inc., 1927. Pp. 95.

The author attempts to determine to what extent the modern psychologists' criticism of the economists' over-rationalization of man is valid. After a statement and examination of the economists' standpoint and of the attack by the psychologists, he concludes that the economists' defense is inadequate.

He then proposes a restatement of economic theory in terms of "demand price" and "supply-price" instead of "utility" and "cost," to avoid questionable psychological implications. This theory is to be supplemented by an inductive, experimental attack on economic problems in place of deductions from any *a priori* view of human nature as the main source of economic knowledge. Offering different wages for various hours of given work and making observations of the laborer's response, is an example of the methods proposed for carrying out this program of reform.

The author's position that the psychologists make man a non-calculating creature driven by instinct, is somewhat extreme and erroneous. He does not appreciate fully the modern view of rational behavior. The disagreement among psychologists as to whether certain universal action-tendencies are instincts or whether they are instincts *plus* habit, is of less importance to economic theory than is maintained in this book. The author is also in error when he lists the emotions of fear, rage, and love as the only "instincts" observed by Watson.

George Peabody College C. W. TELFORD

Human Nature and Education. By A. S. WOODBURNE, New York, Oxford University Press, American Branch, 1926. Pp. ix, 292.

This volume is the outgrowth of a series of lectures apparently to actual or prospective teachers on "Modern Development in Educational Psychology." The traditional categories of general psychology are interestingly presented and numerous applications are made to the problems of teaching and learning. It is rather unusual for a book on educational psychology to give so much attention to philosophical interpretations. A discussion of the freedom of the will alone occupies several pages.

The jewel of consistency seems somewhat dimmed in places. We read, for example, that "there are no such things as a reflex, an instinct, or consciousness. These terms would be much better used only in the descriptive sense as adjectives qualifying the words 'act' or 'behavior' and should not be used as substantives." Yet only four pages later we find that under certain conditions "the hunger stimulus could be responded to by the food instinct without the intervention of consciousness." The latter statement is the more representative of the volume as a whole. An inaccuracy of terminology occurs where, in speaking of experiments on transfer of training, the author says: "one group, called the 'control group', is subjected to special training, whereas the other is subjected as much as possible to identical conditions minus the special training."

The chapters on imagination and examinations are especially good. One valuable feature of the book is found in the numerous glimpses it affords into the educational and social conditions in India.

Kansas State College J. C. PETERSON

The Escape from the Primitive. By HORACE CARNCROSS, New York, Charles Scribner's Sons, 1926. Pp. xiv, 348.

This book is of interest to the psychologist who wishes to recommend to his classes an authoritative, non-technical, psychoanalytic study of human behavior. *The Escape from the Primitive* refers to the escape from the primitive ways of thinking about the order and relation of events in the world, from the system of thought called magic.

The 'primitive' also means the Unconscious where, in the experiences of life, motives are deposited, incubate and mature, determining man's behavior. The psychoanalytic technique cuts into the mental anatomy of the unconscious, exposing false motivation and childish primitive thinking and demonstrating how these prevent the world from growing up and solving the problems of war and peace, of society and the individual, and of progress.

Boston Psychopathic Hospital S. C. ROTH

Common Sense and Its Cultivation. By HANBURY HANKIN. Introduction by C. S. Myers. New York, E. P. Dutton & Co., 1926. Pp. ix, 284.

This book is interesting chiefly to the general reader. It deals with psychological material in an anecdotal, unscientific manner. The author draws a broad distinction between two types of thought; one, intuition or common sense, the other, deliberative reasoning. Intuition has its basis in forgotten memories, which by virtue of being forgotten, are lodged in the subconscious. The fact that intuition is quicker than deliberative thought may, in the opinion of the author, be due partly to the more comprehensive way in which data stored in the 'subconscious mind' are classified and associated. Our modern education is of value to intuition only when it is able to be forgotten and function from the subconscious. A classical education is better in this sense for the business man because its formal nature makes it easy to forget; hence it trains in forgetting, while a scientific education encourages deliberation. The book is replete with incidents from the lives and especially the educational experiences of eminent persons, which incidents the author cites in support of this theory.

Peabody College KATHERINE VICKERY

Guidance for College Women. By MABELLE B. BLAKE. New York, D. Appleton & Co., 1926. Pp. 285.

The subtitle of this book—"A Survey and a Program for Personnel Work in Higher Education"—indicates the intended scope of the treatment, but it was obviously impossible to assemble in this volume more than a small sampling of methods and results of significant personnel projects. Miss Blake has, however, defined the general problem well, formulated principles and answered many objections.

One of her chapters deals cursorily with questions of college admission criteria, another with the relation of secondary school guidance to students' purposes and knowledge of college education.

Various administrative methods adopted for assisting students in early orientation in college, in selection of courses and in occupational choice are briefly indicated. There are also suggested the elements of guidance to be found in student activities which formulate principles of group responsibility and self-discipline.

Results of questionaries of students and alumnae regarding their experiences of need of college guidance, and the findings concerning use of rating scales and records, the functions of personnel directors, faculty advisors, and deans are summarized succinctly.

The opposing policies of personnel administration through centralization or distribution of labor are stated, though not subjected to thorough appraisal. In fact, the book is designed for those who are unfamiliar with the personnel movement in colleges. Its scanty critical consideration of procedures makes it of minor value as a manual or guide to those officers who are actually in the ranks.

Vassar College MIRIAM C. GOULD

The Realm of Mind: An Essay in Metaphysics. By FREDERICK J.E. WOODBRIDGE. New York, Columbia University Press, 1926. Pp. viii, 141.

There are three chapters to this little volume: (1) the realm of mind; (2) objective mind; and (3) many minds. By realm of mind is meant that domain of existence which we inhabit and in which we discover a logical structure. The objective mind is a system of the physical world in terms of which things may be treated as propositions or formulas. The third chapter takes up the individual. A man's mind is not any addition to his body but in fact what he is: he may be analyzed into mind and body along the lines of his activities. This, of course, does not involve mind and body as two distinct and substantial elements of his composition. The third chapter has but little relation to the first two. The book is unified only by its aim to keep the distinction between perceptions and thinking, and physical objects and events, and to put them all in the same 'realm.'

University of Washington RALPH GUNDLACH

Principles in Bio-Physics: The underlying Processes controlling Life Phenomena and Inner Evolution. By C. RICHTER. Harrisburg, Pa., Good Books Corporation, 1927. Pp. 86.

This book is made up of weird speculations expressed in an odd form of symbolization. The author postulates the electrical nature of life-energy and contends that cells which bear the same rate vibrate in resonance. To the psychologist the book has no value except perhaps as one more illustration of the futility of speculation which is inadequately grounded upon the natural sciences.

University of Illinois PAUL THOMAS YOUNG

Source Book for Social Psychology. By KIMBALL YOUNG. New York, Alfred A Knopf, 1927. Pp. xxvi, 844.

The aim of the present work is to present materials which are not easily available to the student and the general reader. A table of contents divides the book into six parts: (1) The general features of social behavior; (2) The psychological foundations of social behavior; (3) Personality and social behavior; (4) Social attitudes and the subjective environment; (5) Leadership and prestige in social behavior; and (6) Collective behavior. There are 27 chapters with a total of 229 sections of reading material. Every chapter begins with a statement from the author descriptive of the materials which follow, and at the close of each chapter there is a list of class assignments with a selected bibliography.

The materials more than cover the field of social psychology. To the reviewer the excerpts seem of rather unequal merit, some quotations having been made apparently to fill out the book. Several case studies from the collection of the author are printed for the first time. Most of the readings are sound, instructive, and interesting. The book is a useful source to place in the hands of students of psychology and sociology.

University of Illinois PAUL THOMAS YOUNG

Educational Psychology. By E. H. CAMERON. New York, The Century Co., 1927. Pp. xiv, 467.

This volume is intended for the more mature student of educational psychology. To the reviewer it seems, however, that the principal function the text can have is to serve the instructor as a suggestive outline of topics to be dealt with in a course. Numerous important questions are mentioned and disposed of in a short paragraph or two, so that it is difficult to see how even the mature student can get an adequate view of the problems involved.

The book does not systematically develop a point of view. It treats the usual topics of original nature, mental measurements, individual differences, nature and nurture, intelligence, kinds of learning, the psychology of common school subjects, and others. Experiments are frequently cited, and there is throughout a marked tendency to think of behavior in terms of 'connections,' although the author does on several occasions speak of learning in terms of patterns (though this word is not employed); but the significance of patterns and meaning in learning is not dealt with. Instead the reader is left with the view that learning is principally a matter of repetition, trial and error, and association.

Cornell University FRANK S. FREEMAN

The Psychology of Mental Disorders. By ABRAHAM MYERSON, New York, The Macmillan Co., 1927. Pp. 135.

An excellent book, written primarily for the layman. The author divides the mental diseases into two groups of major and minor psychoses, and discusses them from the standpoint of etiology, symptomatology, and clinical grouping. Heredity and crime in relation to mental disease and feeblemindedness are dealt with. The subject of eugenics is not neglected, and the author points out how this field needs scientific research more than legislation or publicity.

A program for mental hygiene is presented, emphasizing how prevention, to be of value, must begin with the child. The book is made especially interesting and valuable by the wide experience of the author, whose conclusions are based to a large extent on personal observation and research.

Boston Psychopathic Hospital O. S. ENGLISH

Social Psychology: A Text Book for Students of Economics. By R. H. THOULESS. London, University Tutorial Press, 1925. Pp. vii, 376.

"The present work is an attempt to cover the syllabus of the Psychology sub-section of Sociology in the B.Sc (Economics) Examination of London University. . . . The book therefore ranges widely over the whole field of psychology, developing in greater detail those parts which seem to have the most important bearing on practical problems."

The first half of the book treats chiefly of instincts, following McDougall and the English tradition. The work is differentiated from the ordinary text by the inclusion of two chapters on the psychology of economic values the materials of which would be interesting to students of economics. The latter half of the book also contains chapters on the acquired elements in behavior, sensation and perception, imagery and thought, conflict and volition, the psychology of morality, individual psychology, the psychology of esthetics, and the psychology of scientific and of religious development.

The book is written from a reflective and critical point of view, and is based largely on English psychology with inadequate use of the American and German materials. As a whole the book is clearly written and substantial.

University of Illinois PAUL THOMAS YOUNG

Über Korrelation. By W. BETZ. Leipzig, Johann Ambrosius Barth, 1927. Pp. iv + 65. *Zeitschrift für angewandte Psychologie,* Beiheft 3 (second edition).

This is a revised edition of an earlier work under the same title, which was published in 1911. The only text of its kind in German, it is similar in content to the chapters on correlation in the numerous statistics textbooks in English. It contains, however, some things not ordinarily included in these textbooks, as, for example, a critical discussion of Deuchler's method of rank order correlation and a brief and easily understood statement of Spearman's early work on a "general factor." The statistical subjects presented are product moment correlation methods, the eta coefficient, probable error, rank-order methods of correlation, four-fold tables, contingency, multiple correlation, spurious correlation, and the Spearman work on a "general factor" and the "hierarchy."

University of Chicago THELMA GWINN THURSTONE

Investigations in the Hygiene of Reading. By J. H. BLACKHURST Baltimore, Warwick & York, 1927. Pp. 63.

Chapter I discusses the history of experiments upon reading and also presents data which show changes during recent decades in the height of letters, length of line, and leading of school books. Chapter II describes a method for investigating the most readable size of type, leading, length of line, and marginal identation for children of the first four grades. Chapter III presents in tabular form experimental data upon the problems indicated above. A record was made of the time of reading and of the errors in reading for school children. In conclusion specific recommendations are made regarding the typography of books for use in the first four grades.

University of Illinois PAUL THOMAS YOUNG

History of Ancient and Medieval Philosophy. By H. W. DRESSER. New York, Thomas W. Crowell Co., 1926. Pp. xii, 338.

A practical textbook in philosophy, reviewing the developments from early Greek times to the seventh century.

J. P.

History of Modern Philosophy. By H. W. DRESSER. New York, Thomas W. Crowell Co., 1928. Pp. xvi, 471.

This volume completes the sketch of philosophy begun in the first volume. It devotes a number of pages to philosophy in America, to which certain psychologists (*e. g.* James and Baldwin) have contributed. Of modern writers in Europe and England, Bergson, Croce, Bradley, Bosanquet, Ward, and others receive attention, Croce getting 15 pages.

J. P.

Psychology and Its Use. By E. D. MARTIN. Chicago, American Library Association, 1926. Pp. 47.

This is a popular discussion of some uses and abuses of psychology. The title is rather optimistic for a pamphlet of its length.

Kansas State College J. C. PETERSON

Fear: The Autobiography of James Edwards. By J. R. OLIVER. New York, The Macmillan Co., 1927. Pp. viii, 366.

This book contributes nothing to our knowledge of fear. It is not a scientific treatise, but merely a novel—which the reviewer found to be rather stupid—written in the form of an autobiography, giving the story of the struggles and fears of a typical and common case of psychasthenia. The author is very religious, is embued with an idea concerning the efficacy of prayer, and writes his book with the avowed purpose of showing "exactly where the work of the psychiatrist must stop, and where the work of the priest or the evangelical minister must begin."

K. M. D.

German-English Dictionary of Psychological Terms. By C. A. RUCKMICK. Iowa City, Athens Press, 1928. Pp. 45.

This dictionery gives the English equivalents of over 1200 German words. It will be helpful to the student acquiring a reading knowledge of German psychology. The author has drawn heavily upon the glossaries of Titchener and Judd, and his translations with few exceptions are those generally accepted by psychologists. The addition of a glossary of abbreviations commonly found in the German psychological literature is welcome. The explanation of these abbreviations is nowhere so readily available; the standard German-English dictionaries do not give them, they are not explained in the literature, consequently the student is often at a loss to know where to turn—as indeed must also be true of foreign students over against the abbreviations used in English and American psychology. The author would have done well in this part of his dictionary to have given the German as well as the English equivalents of the abbreviations.

K. M. D.

The Mind and its Mechanism. By P. BOUSFIELD and W. R. BOUSFIELD. New York, E. P. Dutton and Co., 1927. Pp. 224.

Several interesting chapters on the functioning of the "psychic brain." It is not easy to see wherein the author has made Freud's conception of the censor any clearer by postulating in its place an anti-psychogram to replace an unpleasant psychogram. However, in a field so transcendental new conceptions are always welcome; so this is not a severe criticism. Favorable comment is due the authors on their summary of each chapter.

Boston Psychopathic Hospital O. S. ENGLISH

NOTES AND DISCUSSIONS

'HABIT' IN 'APPRECIATION'

Education is concerned with the direction of three types of activities, or better, perhaps, three aspects of activity, in human beings: (1) activities of overt performance—such as speaking and moving; (2) activities that condition or eventuate in overt performance—such as judging and reasoning; and (3) activities that end, or may seem to end, in themselves—such as feeling, insight, or, in general, awareness or consciousness.

So far as education is designed to adjustment of the individual with the physical and social environment in which he lives and of which he is a part, the first two types are significant. Man is, from the point of view of adjustment, an organism in nature and a member of society. As such he *is* what he *does*. His every activity, then, is subject, for valuation, to criteria of efficiency in adjustment. Values subsist in the pragmatic sanction. Education is concerned with conduct, moral, civic, vocational, and the other categories so familiar to us; that is, the ends of education lie in the achievement of organic and social well-doing. Education becomes education for 'efficiency.'

So far, however, as education is designed to the "enrichment of the inner life," whether that be an aspect of adjustment or not, the third type comes to the fore. Man is, from this 'humanistic' standpoint, a self. He *is* in his *awareness*, as well as in his physiological dynamics. Criteria for valuation become subjective and individual, rather than objective and social. Values are inherent, not derivative. The ends of education are in well-being or 'appreciation.'

Experimentation in the functioning of physiological mechanism seems to have furnished a key to method in control of organic reaction. The 'law of use' in formation of habit, or the 'conditioned reflex,' seems to work. Education for adjustment comes into its own, and is on the way to become a 'science' of education. Reactions of overt performance are demonstrably governable by application of the law to educative procedure. We seem to have a method of education for 'efficiency.' Have we in that also a method for education in 'appreciation'?

I. According to the view of very 'advanced scientists' in the field of human behavior, the question is wearily useless. Under given conditions, if a man is conscious, he is conscious as he is conscious; that is all there is to it. Since neither he nor anyone else can observe and describe how he is conscious, scientists need not concern themselves with that. No real loss is involved, since it matters not a whit to the organism or to the world what awareness exists or does not exist in a man under any conditions. There may be 'appreciative' process in one who behaves, but since we can neither know what that is nor what brings it about, we had best cease worrying and stick to our knitting. As we direct successfully the sequences of subvocal response that constitute the intermediary processes of judging and reasoning we are affecting the only knowable insight; as we direct successfully the secretion of the adrenals, the thyroid, *et al.*, which condition capillary contraction and expansion, rate of heart beat, miscellaneous heavings of the entrails, etc., we have, in so far forth, taken charge of the knowable emotions and feelings. For both scientific and practical reasons it is wise to confine ourselves to education for 'efficiency,' and to dismiss the mystical function of education for 'appreciation.'

Under such a view criticism of the 'law of use' as a means to development of activities of the 'inner man' is, of course, without point and futile.

II. A second view admits more of speculation into the behavioristic hypothesis. It suggests the admission of consciousness into the category of behavior. It extends the principle of the conservation of energy to include the supposition that consciousness is a form or manifestation of energy. A feeling, idea, or process of awareness, then, is interpreted as itself a response to some stimulus of glandular or muscular activity; that is, a response in what we call consciousness to a response in the objective physiological organism. Except for being one step removed it stands in the same relation to initial stimulus of a 'chain' as does a movement of the jaw or secretion of saliva. If that is so, however, we should expect the 'law of use' to operate to the establishment of habits of 'appreciation,' just as it does to the establishment of habits of 'efficiency.' Does it so operate?

Among students of human behavior we find agreement upon one law of habit formation—the 'law of use.' The use of a 'conduction unit'—neurone, reflex arc, line of force—for the conduct of 'impulse,' electrical or other, between a receptor mechanism and a response mechanism, affects the readiness of that conduction unit to receive, to conduct, or to discharge in response, when the 'same stimulus' occurs again. Thus a 'habit' of responding in a particular fashion or not responding in that particular fashion may be established. The more frequently the connection is effected the more it is affected to facilitate speed in connection and accuracy in response. Predictability of response from stimulus grows in certainty. That is the 'law of use' in habit formation.

But does this law hold for 'connections' leading to awareness? Experience, not experiment, seems to point to a negative answer. Nothing seems more certain, from subjective evidence—the only kind of evidence that is or can be admissible—than that with 'perfecting' of a habit of bodily response my consciousness thereof or therefrom diminishes or changes in kind. As I perfect my skill in operation of the typewriter, for example, I become less and less aware of keys, of shift, of bell, of touch and movement. In the degree that organism, through habit, takes over the operation, I, as inner man, less and less 'identify myself' with the operation or share consciously in it. Again, I solve a problem in algebra through a series of subvocal mechanistic responses that seem to involve rather acute awareness of certain ideas. But if I repeat my reaction to the same symbolic stimuli those ideas do not recur with increasing certainty and clarity in consciousness. Quite the opposite. Sooner or later the stimuli cease to 'present' a problem in any sense. I react to them with overt correctness and more certainty and speed, but I am less conscious of the ideas that seemed at first to belong to the solution. Habit, again, takes over the problem for me. I, the man who is aware, have less and less to do with it. The 'law of use,' then, accounts for my 'efficiency' in operating the typewriter and in 'solving' the algebraic problem, but it does not account for the difference in my 'appreciation' of the operation and the solving.

Now, whether I speak of the change in 'appreciation' as quantitative—which is, perhaps, not improper in discussing a mechanistic hypothesis—or qualitative, the conclusion I must come to is the same. The 'law of use,' as now formulated and understood, appears not to govern uniformly the conditioning of activities of consciousness as it does appear to govern habits of muscular and glandular response in the organism. There is the suggestion, of course, that use which strengthens the bonds between stimulus and organic response may weaken, in some cases at least, the assumed bonds between organic response acting as stimulus, and awareness as assumed response.

The criticism, then, points to either of two possibilities: (a) the hypothesis that consciousness can be placed in the category of behavior is unsound; or (b) the 'law of use' represents but a special instance of some more

inclusive principle of behavior yet to be discovered. In either case the attempt in educational practice to develop particular appreciations by appreciation of particulars does not find its sanction here.

III. A third view, or rather composite of hypotheses, alike as they bear upon the 'law of use' in education for 'appreciation,' may be stated thus: Consciousness is not response or reaction end. Yet it is related to function in organic mechanism. Consciousness 'arises' as the channels of 'connection' are obstructed. The relation may be causal—which necessitates assumption of a factor of identity in cause and effect; so there may be transformation of the energy of 'impulse' into the energy of consciousness, similar to the conversion of electric current into heat, or light, or motion. Or the relation may be that of incidental but assured concomitance, in which consciousness does not share in the properties of mechanic 'impulse,' but may 'arise' therewith, nevertheless. For discussion here it makes no difference whether awareness is effect or accompaniment or something more mysterious, so long as we grant that it 'arises' with obstruction of the channels of reaction.

To illustrate before proceeding further: Assuming that the neurone is the unit of conduction, consciousness 'arises' with resistance to 'impulse' at the synapse or in the axone. This resistance creates a condition corresponding to that of a dam or partial dam in a stream; against it 'impulse' urges as does water against the dam. In whatever happens, say, an overflow or spreading out, 'impulse' takes on a new aspect which *is* consciousness, just as the overflow of water takes on the aspect of a lake. Or the spreading 'impulse' may *result* in consciousness, just as the overflow of water results in change of color in the soil upon which it flows. Or, again, outspreading 'impulse' is *accompanied by* consciousness, as the overflow of water may be accompanied by frogs, sandpipers, herons, and mosquitoes. The analogies are, of course, crude, but may be, nevertheless, suggestive of concepts of relation between 'physiological process' and 'psychological process' which have wide influence.

Once more, to be sure, the 'perfection' of habit in response at the arc end is, under the 'law of use,' fatal to the conscious elements. Use removes the obstruction, or cuts away the dam. Consciousness sinks as it arose.

There is, however, this comfort for him who would, through habit, enrich the 'inner life.' Before a channel can be obstructed there must be something in the nature of a channel. That is, education for 'appreciation' is still concerned to make 'connections.' To do so involves the 'law of use.' But it is also concerned with the blocking of channels. The implication seems to be that education for 'appreciation' should seek to initiate rather than to 'perfect' habits; it must develop many and branching and relatively shallow channels of organic reaction; not one assured response to one given stimulus, but varied potential responses to that stimulus; not one response to one stimulus, but one response potential to varied stimuli. 'Impulse' must flow as through a "shorn and parcelled Oxus" rather than as through a millrace or a barge canal.

To put the matter in quite different terms, this third view implies—and experience seems to bear out the implication—that consciousness 'arises' in situations involving both the familiar and the novel. To the completely familiar we respond with promptness and accuracy, but automatically and without awareness; to an utterly novel 'situation' we do not respond at all; we have no equipment therefor. In situations between these extremes awareness comes. We respond neither by blind habitual reaction nor by blank inaction, but with 'appreciation' of one sort or another. Habit, or instinct, furnishes the 'familiar' elements in such situations; environment the novel elements. Education for 'appreciation,' then, must work in the middle ground, seeking to open avenues of initial habit that

may function partially in introduction to many predictable *kinds* of situations, rather than completely in certain particular and specifically predictable situations.

It appears, then that direct education in and for 'appreciation' is out of the question under a behavioristic hypothesis. But under the more speculative hypothesis of behavior, indirect education for 'appreciation' is possible. Educational practice guided by the 'law of use' to ends far short of fixed arcs of reaction, may contribute to 'enrichment of the inner life.'

Cornell University THEODORE H. EATON

THE GEORGIA EXPERIMENT

Psychology at the University of Georgia is ranked among the natural sciences. It is included in the science group together with physics, chemistry, and biology. This has been brought about by the fact that all the undergraduate courses in psychology are taught in conjunction with the laboratory. All the courses, of which there are three, run three quarters. Four hours of university credit, equivalent to eight hours semester credit, is given for every course each year. In the elementary course three 1-hr. periods a week are given to the class-room—to lectures, discussions, and examinations—and one 2 to 2 1/2-hr. period a week is given to the laboratory. The advanced undergraduate courses have three 2-hr. laboratory periods and one conference a week.

The class in the elementary course is divided for the laboratory work into sections of approximately 20 students each. The limit for a section is placed at this number so as to lighten the burden upon the instructor, and also to reduce the amount of duplicate apparatus needed.

Two special aims underly the course: (1) the student is required to do his experimental work as nearly as possible under conditions of research; and (2) he is thrown so far as possible upon his own resources and is required to do all that is possible by himself in place of merely following instructions and verifying statements.

The accomplishment of these aims is attempted with the help of a manual written especially for the purpose, and the procedure in the course involves certain essential features which include the following: (a) discussion at the beginning of the year of scientific method with special emphasis upon sources of error and reference to books which are to be studied in this connection. (b) Study and discussion with the instructor of two or three sample reports well done by former students which are constantly used for reference in writing up reports. (c) Instructions and an outline for writing up experiments as follows: statement of the problem; method, materials, and apparatus; data or results of experiment; sources of error; conclusions; theoretical explanation; answers to questions. (d) The use of 'originals' when students have progressed sufficiently.

The discussion of scientific method is for the purpose of helping the student to a scientific attitude and to insight into the care with which he should work. The selected reports of former students set a standard for the writing up of reports and help the students to understand what is required. The instructions to students including the outline for writing up the experiment help them to a systematic organization of their work and to a clearness of thinking. The most difficult part of this work is to get the student to keep his conclusions clear of his statement of results of experimentation. Conclusions are accepted only if they agree with the experimental data whether or not they agree with the textbook statements. Any variance from textbook statements is to be indicated. Theoretical explanations are to be quoted from books which are in the laboratory,

such as those of Ladd and Woodworth, Pillsbury, Dunlap, Titchener. All graphs, tables, legends, etc., are to be done carefully and as if the report were to go to the printer.

Thirty experiments are the minimum for the course although more than thirty may be done. As given at the present time the thirty experiments are selected from the fifty-five indicated below. The first thirty-eight are regular experiments, the remainder are originals.

Measurement of error in Müller-Lyer and size-weight illusions;
Drawing of brain and nervous system;
Learning in finger maze;
Mirror drawing with star;
Auditory acuity;
Description of visual experiences with brightnesses and colors, including description of course of after-image;
Retinal sensitivity: color zones and blind spot;
Study of visual contrasts and the vision octahedron;
Drawing of the eye and its parts;
Mapping of cold and warm spots;
Mapping of touch spots;
Perception of illusion of movement and drawings of the ear;
Span of attention;
Stereoscopic vision: perception of depth, retinal rivalry;
Auditory perception of direction of sound;
Two point limen;
Imagery: types of imagery, vividness of imagery;
Visual illusions;
Associative connection;
'Free' or 'uncontrolled' association;
Tapping associations;
Psychology of report;
Memory span;
The conditions of interest;
Judgment of the duration of time;
Judgment of distance;
Judgment of emotion from photographs;
Reaction times;
The psychogalvanic reflex;
Judgment of intelligence from photographs;
Ability to follow directions;
Study of an intelligence test;
Statistical study of results of intelligence test;
Study of another test, as an achievement test;
Study of correlation;
Study of another test, as ethical discrimination or will-temperament;
Information test;
Vocabulary test.

The originals include the following:

Indirect vision;
Apparent size of objects that stimulate the skin at different places;
Binocular and monocular vision;
Modification of a reflex;
Unconscious action;
Set or disposition;
Methods of memorizing;
Meaning and retention;
Individual differences;
Correlation *vs.* compensation;

Affective value of colors;
Taste perceptions;
Judgment of human traits;
Improvement with and without knowledge of successes and failures;
Division of time in learning;
Learning and definiteness of practice.

Provision is also made for one long experiment as in learning. Few students, however, are permitted to take a long experiment. The writer has tried giving beginning students long experiments and has come to the conclusion that it is better to give one experiment a week for the following reasons: interest is better maintained; the one long experiment (although the student learns more in a limited field) may incline him to think that he does not like psychology, whereas, with a number of experiments, interest is always developed in connection with some of them; a short series of experiments can be correlated much better with the lectures and discussions and there is better understanding of all subjects discussed after the laboratory work has been done in connection with them.

The selection of experiments has been made with the following points in mind: practicability for a 2 to 2½-hr. period, expense of duplicate apparatus, correlation with lectures and discussions, amount of psychology that students might learn, introduction of student to various types of experiments and tests.

One of the features of the course is the giving of a minimum of instructions for experimentation. This is for the purpose of requiring the maximum of understanding and work on the part of the student. The preliminary talk and verbal instructions with demonstration by the instructor at the beginning of a laboratory period supply for any given experiment what the instructor judges that the students need in addition to what is in the manual. At the first of the year more instructions are given and later they are minimized. The student cannot blindly follow instructions; he is forced to know what he is doing and to formulate for himself his problem and method. The originals are used only after the student has progressed sufficiently in the judgment of the instructor and he is then helped as little as possible. All work must be finished and reports handed in before the students leave the laboratory.

The advantage of this course may be summarized as follows. (1) Demanding work to be done as nearly as possible as it is to be done in advanced courses and in research work. (2) Requiring the statement of the problem in the student's own words and his own statement of methods. (3) Demanding effort at solution of some problem instead of following detailed instructions point by point. (4) Demanding that the student draw conclusions from the facts obtained by experiment. He must be true to his facts. He must learn to go where the facts take him. Here is training in one of the most fundamental traits of scientific work. There is no science without facts; neither is there any science without logic; from one point of view, science is facts plus logic. In this course the facts must be obtained under experimental conditions; the conclusions must follow with logical necessity from the facts. (5) Demanding attention to the sources of error in the experiment. (6) The use of originals which demand and develop ability to apply what has been learned of scientific method. (7) Complete and thoroughly written reports of each experiment as nearly as possible as it would be done for a journal article—graphs, legends, figure numbers, etc., must be included and done as they are done in an acceptable reference book or a psychological journal. (8) Students are forced to study the best texts for theoretical explanations and thus gain familiarity with them.

The course may, on the other hand, be discouraging to the beginning student unless the instructor encourages him and helps him to under-

stand the value of such work. Another possible disadvantage may appear in the length of the written reports. This is not wholly bad though it is sometimes wearisome to the student.

Comparing the results of about 100 students a year for three years with students of earlier years, there appears to be four points of superiority in the results of the present plan: (1) greater ability to recognize and to state a problem definitely; (2) to draw conclusions from data; (3) to keep conclusions properly separated from the results of experimentation; and (4) greater familiarity with texts used in the laboratory. Graduate students show the effects of this training as compared with students who have not had it. A number of former students have volunteered the statement that they can apply the methods learned in this course to the solving of problems in other situations.

The classicists have challenged the ability of science to give adequate training to our youth. Experimenting is the highest type of learning as it includes all other kinds. Through experiment and the improvement of our experimental courses science may demonstrate the possibility of making the best contribution to the student's education.

University of Georgia A. S. EDWARDS

FERTILITY AND PARENTAL INTELLIGENCE

It has been pointed out that the principal determinations, such as those of Lentz and of Sutherland and Thompson, of the correlation between fertility and intelligence have considered only *offspring* intelligence; this has been due, of course, to the fact that it is ordinarily only children who are available for testing. Though it seems *a priori* unlikely that the values of these correlations would be greatly altered by using parental intelligence, the complete analysis of the problem certainly requires their experimental determination.

Opportunity may be made for a crude approximation to the desired figures by reëxamining the writer's data on about 100 California families, collected in 1924 for the purpose of determining the values of the familial coefficients of resemblance.[1] These data share with those of most similar studies the defect that it is impossible to take into account from them the childless portion of the population; on the problem of incomplete families they are perhaps somewhat better than most, since the criterion of the inclusion of a thirteen-year-old in each family brought the average age of mothers to the vicinity of 42 or 43. The nature of the test used (paper-and-pencil variety) tended to lower the frequency of record both of illiterate (*i.e.* fecund) families and younger children; the latter error has been corrected in the present calculations wherever possible. More serious is the impossibility, for personal reasons, of deriving the usual child intelligence-fertility correlations from the same data for purposes of comparison.

It was found possible to combine the scores for the 11 tests of the battery (6 verbal, 5 non-verbal tests) in to a single score, which was not attempted in the original monograph. This yields what must be regarded as a fairly adequate measure of general ability of the intelligence-test variety. The procedure in this was not strictly orthodox; it was made possible by the fact that the age curves for both means and standard deviations of all tests descend at a negligible angle through the range populated by practically all the parents. It consisted in multiplying or dividing the raw score on each test by such an integer as was necessary to bring

[1] R. R. Willoughby, Family similarities in mental-test abilities, *Genet. Psychol. Monog.*, 2, 1927, 235-277.

all the age curves above-mentioned to substantial congruence, and then adding the 11 results (or, in the case of N tests—$4 < N < 11$—adding the N results and multiplying by 11/N). Since by hypothesis all the standard deviation curves of the converted scores approximately coincide, this amounts to weighting the 11 tests equally, which seems to involve no unreasonable assumptions. These combined scores constitute one variable in the correlations obtained; the other is the number of children for whom record was secured, corrected in some cases in which it was known that there were other children. The few cases in which the families were probably incomplete were included, after it was ascertained that that their inclusion resulted in no appreciable effect on the correlations.

The Pearson correlation coefficient between mother's intelligence and number of children was -0.11 ± 0.06; that between father's intelligence and number of children was $+0.06 \pm 0.07$. The former is based on 108 cases and the latter on 87. The sources of error mentioned above make it unfruitful to generalize very extensively from this result; but it is noteworthy that even with fairly liberal allowances for error the figures would still be surprisingly near zero. Keeping in mind the augmenting effect of assortative mating recently shown to follow from multiple-factor assumptions,[2] it appears increasingly probable that the assortative mating coefficient (0.40 to 0.60) and the fertility-intelligence coefficient (-0.10 to -0.20) form a balanced system, the effect of which is to keep the population intelligence mean approximately constant, or at least subject to determination by local accidents or by other factors. There seems to be, as a matter of fact, no adequate evidence for any historical change either in the degree of the value of the fertility-intelligence correlation, assortative mating, or the level of the population mean.

Of passing interest are the constants of these distributions: The mean number of children for both fathers and mothers is 2.42, and the standard deviation is 1.17; the coincidence in the case of both figures is of interest as showing that it is not a fertility factor which eliminates the fathers (many of whom are deceased). The average intelligence score of the mothers was 100.2, and that of the fathers 108.0; the difference of 7.8 has a standard error of 4.8, corresponding to odds of about 15 to 1 in favor of a real difference. The standard deviations of the parental intelligence scores were 32.3 for the mothers and 35.1 for the fathers; the difference of 2.8 has a standard error of 3.5, and so is only apparent. The difference between the parental fertility-intelligence coefficients (0.17) has a standard error of 0.14, corresponding to odds of about 8 to 1 in favor of a real difference. The suggestion implicit in this last finding, viz., that the effective determinating factor of the negative fertility-intelligence correlation itself may be the superior ability of intelligent women to anticipate in imagination the trials of motherhood, has not, to the writer's knowledge, been advanced before, and seems a promising field for further investigation.

Clark University RAYMOND R. WILLOUGHBY

THE THIRD GERMAN MEDICAL CONGRESS FOR PSYCHOTHERAPY

The third medical congress for psychotherapy, which was held this spring in Baden-Baden, had like its predecessors two principal aims: first, to give the general medical practitioner some insight into the problems of psychiatry; and secondly, to clear the way for mutual understanding between conflicting schools of psychotherapists.

This time the two schools struggling for supremacy were the individual psychology of Alfred Adler and Freud's theory of psychoanalysis. In-

[2] R. R. Willoughby and Goodrie, Neglected factors in the differential birth rate problem, *Ped. Sem.*, 34, 1927, 373-393.

dividual psychology was represented by the reports of Seif (Munich), Wexberg (Vienna), and Künkel (Berlin). Their psychoanalytical opponents were Schultz-Hencke (Berlin) and Schilder (Vienna).

Seif gave an outline of the point of view of individual psychology with regard to the problem of neurosis. According to him the neurosis is a disturbance of interhuman relations, an excuse and an effort to justify the discouraged, egocentric individual in escaping from his lifework. Accordingly, the cure consists in educating the discouraged and dependent individual to become a courageous and independent one, by converting his egocentric attitude into an appropriate one which makes him fit for life.

Wexberg reported on organic inferiority in connection with certain results of internal medicine. According to his view Adler's doctrine of organic inferiority represents an extension of the principle of the point of minor resistance to the sphere of clinical pathology. Whilst the doctrine of constitution assumes partly the conception of a specific disposition for certain diseases, partly that of a constitutional debility of the whole organism, Adler asserts a predisposition of certain organs and organic systems to diseases of different kinds. There are families for instance, with a disposition to debility of the alimentary tract, which manifests itself in one case by an *ulcus ventriculi*, in another by a stomach carcinoma, in a third case eventually by a dyspepsia. Wexberg thinks that this connection is nowadays often accepted in constitutional research as well as in internal medicine. Less attention is paid to another section of Alder's doctrine, the compensation of organic inferiority. This supercompensation changes an inferior organ not into a normal one, but into an 'emphasized' one. The psychologically important consequence of the compensation of an organic inferiority is that eventually the entire organism seems to be grouped around the emphasized organ as its center.

Schultz-Hencke, one of the psychoanalytic speakers, asserted that all theses of individual psychology are implicitly contained in psychoanalysis, while the reverse is not true. Individual psychology merely inquires whither the symptoms are tending, whereas psychoanalysis also searches for causes. He does not deny that individual psychology is giving satisfactory service in the range of what psychoanalysis calls "the secondary profit of disease," that is, demonstrating the actual psychical situation which favors the fixation of symptoms. But its range is far narrower, considering the enormous extension of the field, than that of psychoanalysis. Schilder analyzed Wexberg's report and gave a fundamental criticism of Adler's doctrine of organic inferiority. He pointed out that Adler's doctrine is, using a term of modern Gestalt-psychology, typical 'piece-work' (*stückhaft*). The inferior organ is not isolated in the organism. Temperament, strength of the affects, and of the intellect of the entire personality, determine how it shall manifest itself. The problem of constitution goes far too deep to be capable of solution by the formula of organic inferiority.

The second subject of discussion was the problem of character. What is character; how is it to be conceived? Häberlin (Bale), Allers (Vienna), and Klages (Zürich) tried to investigate these intricate problems.

Häberlin pointed out the tasks confronting scientific characterology, and the means by which it can solve them. The principal task of characterology is to formulate a psychologically and logically ordered system of the points in which two individual characters may differ. Characterological differences must follow the connection of actions. This connection is described first by its form, secondly by its content. Formal characterological features are, for instance, the constancy of character, its differentiation, the course of action, intelligence, memory, etc. The content is the interest that manifests itself in the individual's actions.

Allers too asserted that understanding of character must be based on analysis of its actions. Every action tends towards a definite result which always consists in establishing an entity. Further, the action is always the releasing of a tension out of which it has arisen, and which it neutralizes. According to Allers character cannot be understood from the structure of impulses, neither does he admit that it is determined by innate constitutional factors. He defines character as the formal principle of the total of all reactions of a human being. Since action tends towards values, character may be defined as the substance of individual laws of value-preference.

The third subject of the medical congress of psychotherapy was the relations between experimental psychology and psychotherapy. Lewin (Berlin) pointed out in an extremely interesting paper that experimental psychology has now become capable of treating successfully problems of emotional and impulsive life, of essentials importance in therapy. The experiment need not aim at imitating definite situations in real life; it is possible experimentally to construct the dynamical equivalent of such situations. It is even possible within the scope of laboratory experiments to arrange situations where the person tested is not aware of being part of the experiment; experiments exempt from task. Lewin described experiments aiming to explain tension and detension of need-systems, the degree of communication between different needs and psychical spheres, the question of repression, the different kinds of surrogate gratifications; experiments which have already produced enlightening results.

A series of lectures completed the program. Keen discussion after the lectures proved the active interest taken in these questions by the German medical world.

University of Vienna GEORGE GERÖ

DO AMERICAN PSYCHOLOGISTS READ EUROPEAN PSYCHOLOGY?

In 1925 the Research Information Service of the National Research Council issued, for 114 American libraries, a union check list for 194 foreign serials. These serials are the most important for psychologists in the sense that they are those listed in the *Psychological Index* for 1922. The list of 114 libraries consists of 31 public libraries (including the libraries of academies, societies, institutes and museums, not connected with universities) and 83 university or college libraries (including university museums and medical schools).

For the purposes of this note I have selected the nine psychological journals that I should suppose to be most generally read by general and experimental psychologists in America, and I have added one Italian journal for interest, although most Americans do not read Italian. Here follows the frequency of the occurrence of these journals in the 114 libraries:

Mind	82
British Journal of Psychology	69
Brain	56
Archiv für die gesamte Psychologie	53
Zeitschrift für Psychologie	50
L'année psychologique	38
Archives de psychologie	37
Zeitschrift für Sinnesphysiologie	35
Psychologische Forschung	15
Rivista di psicologia	11

The three British journals head the list. English is favored although German psychology has long led British psychology. It is quite proper, however, that German serials should be more readily available than French;

nor should it be surprising to find the Italian journal last, though the psychologist who is forced to use it (having come to it perhaps by way of the *Psychological A' stracts*) does not find himself very well served.

It is also apparent that the older journals have the advantage. In 1925 everybody in America had for a few years been discussing *Gestalt-psychologie*, and yet the *Psychologische Forschung*, then in vol. 6, was available in only 3 universities in New England, 4 in the Middle States, 5 in the Middle West, one in the far West, and in two libraries of scientific societies. Could a recent graduate student, who had been placed anywhere but in one of these 13 universities, be expected to know anythng much at first hand about this all-important new movement in German psychology and its experimental work?

It is safe to say that very, very few American psychologists, young or old, subscribe personally to these foreign journals. Not only are they too expensive for the younger men; but statistics show that very few members of the American Psychological Association, which includes nearly all of the psychologists we are discussing, subscribe for any of the Psychological Review Publications or for this JOURNAL. Psychologists who do not subscribe at home are unlikely to subscribe abroad.

What then do my data mean? They mean that a psychologist placed at one of these 82 academic institutions (and the list includes nearly all the large universities and is representative of the colleges) has an excellent chance of finding in the library *Mind*, which he will seldom want, a good chance for the other British journals, less than an even chance for any German journal, still less for the French, and only about a 15% chance for a new, important German journal in its fifth year.

I am not saying that American psychologists would (or could) ordinarily read German if they had it to read. They might read it more if they had it. They might have it more if they demanded it. I am saying that they probably do not read it very much. No wonder *Psychological Abstracts* has to translate into English. A psychologist asked me the other day why this JOURNAL does not print more foreign articles and print them without translation. Here is the answer. Here too, I should say, is the reason for scientific provincialism, not only, I think, in America, but also in Britain and Germany. Perhaps there is nothing to do about it, unless the fault and the remedy lie in our graduate schools.

Harvard University E. G. B.

A CORRECTION

In his *Introduction to the Effects of Music*, a volume of studies by various authors, edited by the undersigned, Dr. W. V. Bingham, in referring to the Edison Competition, states on page 5: "Judged on such a basis, the choice rested on the admirably crisp and clear-cut report of experiments by Professor Margaret Floy Washburn and her student assistants at Vassar College. This prize-winning study on 'The Effects of Repetition' is found on pages 199 to 210." This statement is incorrect. The prize-winning study is the joint work of Dr. Washburn and Professor Dickinson, on "The Sources and Nature of the Affective Reaction to Instrumental Music," constituting Chapter VI of the book.

Carnegie Institute of Technology MAX SCHOEN

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By J. P. GUILFORD, University of Nebraska

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